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# CLASSICAL PHILOLOGY

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in the Languages, Literatures, History,  
and Life of Classical Antiquity   •   •

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# CLASSICAL PHILOLOGY

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# Classical Philology

VOLUME XIX

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NUMBER I

## THE ORIGIN OF THE SYLLOGISM

BY PAUL SHOREY

The Platonic dialectic anticipates nearly everything in the Aristotelian logic except the explicit exposition of the syllogism. Plato, of course, has the word *συλλογίζεσθαι*<sup>1</sup> but not quite in the Aristotelian sense. Many virtually complete syllogisms can be picked out of dialectical arguments in Plato, especially in the *Parmenides* and Book I of the *Republic*, and some of the later Platonists assumed his acquaintance with the syllogism, its moods and figures. Alcinous, for example,<sup>2</sup> gives specific instances for each one of the three figures.<sup>3</sup> The more critical recognize that this was reserved for Aristotle. Aristotle himself was very proud of the discovery, which he tells his audience cost him much toil, and in which he boasts that he had no predecessor.<sup>4</sup> It must have come early in the series of his extant

<sup>1</sup> Cf. Gorgias 479C and for further details, Prantl, *Abhandl. Munich Acad.*, VII, 208, n. 155.

<sup>2</sup> Teubner *Plato*, VI, 158.

<sup>3</sup> Philoponus on *Ar. An. Prior*, *Wallies*, p. 6, l. 14, reports that Themistius in his commentary on the *Prior Analytics* maintains that the substance of logic is Platonic and only the technical arrangement belongs to Aristotle: "For it is apparent that the divine Plato proceeds syllogistically and apodeictically in the *Phaedo* and in almost all his other dialogues." By pressing *ἐν τῷ Φαιδῶνι* I might represent Themistius as anticipating the results of this paper.

<sup>4</sup> *De Sophist. Elench.* in fine.

writings, for it is taken for granted in the *Topics* and in the *Rhetoric*.<sup>1</sup>

This deprives of all basis Maier's affirmation that the treatment of definition in the *Topics* must be later than that in the *Prior Analytics* I. 31, and the *Posterior Analytics* II, and his opinion that I. 31 of the *Prior Analytics* must have been added after the completion of *Posterior Analytics* II. His arguments overlook the fact that the unity of the definition, the exact nature of essential definition, and its relation to proof remained a problem for Aristotle throughout the *Analytics* and was taken up again with reference to the *Analytics* in the *Metaphysics*.<sup>2</sup> How was the discovery made? I do not mean, how did he work out the detail of the moods and figures. This was done largely by laborious experiment and verification, supplementing à priori reasoning; and Maier's account of the matter may be provisionally accepted.<sup>3</sup> But whence came the first idea? Aristotle does not tell us, and we must divine.<sup>4</sup>

The prevailing view,<sup>5</sup> for which I propose to substitute another, is that the syllogism was suggested to Aristotle by the Platonic *διαίρεσις*, or continuous division by dichotomy, best known from its

<sup>1</sup> Conjectures as to the relative dates of the *Topics* and *Analytics* or particular books and chapters in either do not affect this statement. The *Topics* as a whole assumes the syllogism, recognizes the distinction between scientific proof and dialectical argument, and explicitly refers or postpones a more precise examination of the use of the syllogism in definition to another treatise. *Top.* vii. 2. 2: *ὅτι δὲ ἀκριβέας μὲν ἄλλης ἐστὶ πραγματείας*, etc. So also *De Interp.* v, on problem of unity of definition.

<sup>2</sup> Cf. *De Interp.* v. 3, *Analyst. Post.* II, vi. 6, II, ix. 2. *Met.* 1037 B 8: *νῦν δὲ λέγωμεν πρῶτον, ἐφ' ὅσον ἐν τοῖς ἀναλυτικοῖς περὶ ὁρισμοῦ μὴ εἰρηται.* Maier, *Syllogistik*, II, 2, 80. The *Topics*, dealing with the dialectical use of the syllogism, could not be expected to discuss this metaphysical puzzle.

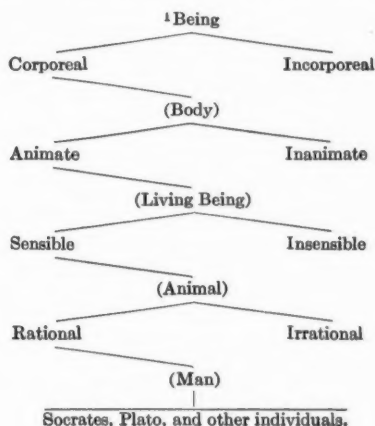
<sup>3</sup> Cf. Leibnitz, *Nouveaux Essais*, IV, xvii, 4: "Mais cependant ces formes ne sont pas le seul ny le meilleur moyen de raisonner: et Aristote ne les trouva pas par le moyen des formes mêmes, mais par la voye originale de la convenance manifeste des idées."

<sup>4</sup> Prantl, *Abhandl. Munich Akad.*, VII, 201, shows that nearly all of Aristotle's explicit references to Plato are polemic. His indebtedness to his teacher he leaves us to discover.

<sup>5</sup> To the older and the German critics add Jowett 1. 192: "The form of the Syllogism is in the genealogical trees of the Sophist and Statesman"; and Ross, *Aristotle*, p. 32. The nearest, perhaps, had been Plato's formulation of the processes of logical division.

reproduction as Porphyry's logical tree in handbooks of logic.<sup>1</sup> Aristotle's frequent references to this Platonic method and the fact that the *Sophist* and *Politicus*, the two dialogues in which it is most fully expounded, were written about the time when he entered the Academy makes this origin of the syllogism plausible. In my *Unity of Plato's Thought* (pp. 50-51) I have shown that the significance of this method for Plato's own thinking has been greatly exaggerated. Plato always practiced division for the distinction of the equivocal meanings of words and, as in the *Gorgias*, for the setting up of such classifications as his argument required. In the *Sophist* and *Politicus* he amused himself by elaborating the process into a formal method of definition by exhaustive subdivision, which he proclaimed no object of inquiry could escape.<sup>2</sup>

But apart from his recommendation of the method, as a means of observing all relevant distinctions, it is plain that he is jesting. And in the *Philebus*, the *Timaeus*, and the *Laws*, his references to the method of division are in the tone of the earlier dialogues, and not in that of the triumphantly proclaimed new method. But the advertisement of the method in the *Sophist* and *Politicus* naturally attracted attention. The divisions of the Platonists were parodied in



<sup>2</sup> Soph 235C.

the new comedy, and came to be considered throughout later antiquity as the counterpart, in Platonic logic, of the syllogism of Aristotle.<sup>1</sup> With all this we are not further here concerned.

Aristotle is much preoccupied with the Platonic diaeresis. He is especially eager to show that, whatever its uses, it does not syllogize, does not prove anything, and in particular, that it does not prove, but at the most assumes, a definition.<sup>2</sup> Was it none the less the source of his own doctrine of the syllogism? There is no direct evidence. We have only probabilities to guide us. The chief consideration, as I hope to show, is that the syllogism has another origin, that it was suggested by a well-known passage of Plato's *Phaedo*. Meanwhile it is enough to point out that though the logical tree of subdivision presents concepts that stand in the relation of whole and part, as in a syllogism,<sup>3</sup> the actual formula of the syllogism is very awkwardly extracted from it, and there is no evidence that Aristotle proceeded in this way.

To take the beginning of the first series in the *Sophist* (219), art is either production or acquisition. Acquisitive art is either an art of exchange or of conquest. The most obvious syllogism to be extracted from this would be: All arts of conquest are arts of acquisition; all arts of acquisition are arts—a most unnatural and useless direction for thought to take. This is a very simple illustration, but it would add nothing to include the entire series of dichotomies down to the art of fishing. As Aristotle says, the process is the same whether the links are many or few. Aristotle himself speaks of diaeresis as a weak syllogism,<sup>4</sup> and the determination of his precise meaning is not essential to our present purpose. Division takes for granted, he says, rather than proves, what it selects at every step, which Plato would have admitted.<sup>5</sup> Indeed, when they speak seriously, Plato and Aristotle describe the use of the method in the same way. It abbreviates the process of search by progressively

<sup>1</sup> Cf. Alcinous: *εἰσαγωγή* V and the frequent references in Cicero to *partitio*, e.g., *Acad.* 1. 2, *nilhil partiuntur*. Cf. Plotinus *Ennead.* i. 3. 4.

<sup>2</sup> Cf. *Part. An.* I. 2. 3, *An. Prior* 1. 31, *An. Post.* 2. 5. 6, *συλλογισμὸν δ' οὐ λέγει ὁ ἐκ τῆς διαιρέσεως ἐκλέγων τὸν ὀρισμὸν*. Cf. also Zeller, *Aristotle* (English translation), I, 241, 267; II, 86; Hicks on *Ar. de An.* 402 a 19.

<sup>3</sup> Cf. *infra*, pp. 13–14.

<sup>4</sup> *An. Prior* I. 31.

<sup>5</sup> Cf. Philoponus, *Wallies*, 307. 5: οὐ μέντοι γε ταύτην ἀποδεικτικὴν λέγει εἶναι.



confining the view to a smaller number of things.<sup>1</sup> Aristotle further says that diaeresis syllogizes something higher up, by which he seems to mean that starting lower down in the logical tree, it syllogizes back to the conclusion that the term selected falls under the alternative of the dichotomy that precedes it; that is, for example, that man, since he is being, either is corporeal or incorporeal being; and, since he is animal, is either rational or irrational animal. Whatever Aristotle's meaning, there is little to suggest either the fundamental conception or the terminology of the syllogism in this far-fetched comparison of it with diaeresis.

Lutoslawski's<sup>2</sup> reinforcement of the argument by the discovery of the middle term in Plato's *Philebus* 17A is a complete misconception of the *Philebus* passage.<sup>3</sup> And the bare possibility that Plato's use of μέσα here suggested to Aristotle his use of "middle" term, even if conceded, would not prove that the syllogism was derived from dichotomy. Aristotle's own statement is that diaeresis treats the universal (e.g., Being) as the middle term, reversing the true method.<sup>4</sup> Maier, who affirms that without doubt the syllogism owes its discovery to Aristotle's critical examination of the process of division, offers no evidence.<sup>5</sup> He merely argues that the failure of the Platonic

<sup>1</sup> *Polit.* 262a: τὸ ζητούμενον ἐν διπλασίοις τὰ νῦν ἐν τοῖς ἡμίσεσι εἰς τότε ποιήσει ζητέσθαι. Cf. *Ar. An. Post.* II. 12. 8; *Topics* II. 2. 2, σκοπεῖν δὲ κατ' εἶδη, καὶ μὴ ἐν τοῖς ἀπείροις· ὁδὴ γὰρ μᾶλλον καὶ ἐν ἐλάττωσιν ἢ σκέψις. Cf. Leibnitz, *Nouveaux Essais*, III, iii, 10: "Et toutes les divisions pourroient estre reduites à des dichotomies, qui en sont la meilleure espeece et servent beaucoup pour l'invention, le jugement et la memoire."

<sup>2</sup> *Origin of Plato's Logic*, p. 464.

<sup>3</sup> Plato is merely warning young metaphysicians against the temptation to fly at once from the infinite detail of concrete things to the highest generalizations, in disregard of intermediate species and genera. Cf. Bacon, Ellis, and Spedding, VIII, 42: "For hitherto the proceeding has been to fly at once from the sense and the particulars to the most general propositions, etc."; and Emerson's *Plato*: "A too rapid unification and an excessive appliance to parts and particulars are the two dangers of speculation." Sir James Mackintosh, *Dissertation on the Progress of Ethical Philosophy*: "The inconvenience of leaping at once from the most general laws, to a multiplicity of minute appearances." Mill, *Dissertations and Discussions* III. 143: "Without such middle principles an universal principle . . . serves for little but a thesaurus of common-places. In the example before Plato either to say that there are infinite pleasures or to insist that all pleasures are simply of the one genus pleasure, is to overlook the distinction between different kinds or species of pleasure. It is of relevant distinctions that Plato is thinking mainly, not of a formal process of reasoning." *Soph.* 227C, μόνον ἐξέτω χωρὶς.

<sup>4</sup> *An. Prior.* 1. 31.

<sup>5</sup> *Syllogistik des Aristoteles*, II, 2, 77.

method to yield necessary conclusions led Aristotle to look for a logic that would, or again, which amounts to the same thing, that having shown that diaeresis did not furnish proof, he went on to seek for a method that did. But the insistent and somewhat invidious<sup>1</sup> testing of the Platonic diaeresis by the syllogism reads more like the polemical comparison of two finished and competing methods than the record of the process by which Aristotle felt the way to his own discovery.<sup>2</sup> And it is a very feeble evasion of this objection to say, as Maier does, that even if this criticism of diaeresis is actually later than the discovery of the syllogism, the discovery may have been due to it. That is of course possible. But we want something more than a bare possibility. The Aristotelian syllogism,<sup>3</sup> as we shall see, is also a doctrine of causality, and there is little to suggest that in the diaeresis.

If, then, we can find another Platonic method that directly yields the formula of the syllogism, and that is a theory of causality, the presumption will be that it rather than the diaeresis gave to Aristotle the suggestion of the syllogism. Such a method we have in what Plato ironically calls the safe and simple method of ideas,<sup>4</sup> which in the *Phaedo* he substitutes for all other systems of causality. The discussion of causation that precedes this passage explicitly states three things: Plato's justifiable scepticism about the efficient causes proposed by the imperfect science of his day, his preference for the final cause—for the transference, that is, of the final cause from man to nature as a whole, his inability to discover any such philosophy of final causes as that which he later presented in the *Timaeus* in a mythical and poetical form. It is because of this inability that he

<sup>1</sup> *An. Post.* II. 5. 1: ἀσυλλόγιστος μὲν οὖν καὶ ἡ χρῆσις γίνεται τοῖς οὕτω μετιώσει καὶ τῶν ἐνδεχομένων συλλογισθῆναι. Wallies, 306. 31: βούλεται διὰ τούτων ἐξυμῆσαι τὴν παρ' αὐτοῦ παραδεδομένην μέθοδον, Philoponus says.

<sup>2</sup> *An. Prior* I. 31. 5: φανερόν οὖν ὅτι οὔτε πρὸς πᾶσαν σκέψιν ἀρμόζει τῆς ζητήσεως ὁ τρόπος (i.e., the diaeresis), οὐτ' ἐν οἷς μάλιστα δοκεῖ πρέπειν, ἐν τούτοις ἐστὶ χρῆσιμος.

<sup>3</sup> *Infra*, pp. 8-9.

<sup>4</sup> *Phaedo* 100C: τὰς ἄλλας αἰτίας τὰς σοφὰς ταύτας . . . ἀτέχνως καὶ ἴσως εὐθύως ἔχω παρ' ἑμαντῶ. 105BC: τὴν ἀσφαλῆ ἐκείνην . . . ἐκείνην τὴν ἀμαθῆ . . . ἀλλὰ κομψοτέραν. The use of ἀμαθῆ and κομψοτέραν here shows that Plato is jesting—that is, that he feels as fully as any of his modern critics the humor of the tautologies of elementary logic when illustrated by the simplest examples. Prantl, *Munich Abhandl.*, VII, 198, recognizes the connection of "presence" and causality in Plato, but gets no further. *Ibid.*, p. 203, he evidently regards διαίρεσις as the origin of the syllogism.

falls back upon the second best thing,<sup>1</sup> the simple method of ideas. This is so obviously what Plato does say in the *Phaedo* that I should have thought it superfluous to call attention to it, if it had not been repeatedly overlooked or denied by a large proportion of critics, some of whom even go the length of identifying the method of ideas with the desired but undiscoverable method of final causes. For this reason, and also to indicate that I had already reached the conclusions of the present paper, I explicitly interpreted the *Phaedo* passage in my doctor's dissertation, and from time to time reaffirmed the interpretation in subsequent papers.<sup>2</sup> And I feel justified in mentioning, though not by way of complaint, that Professor Ritter and others who of late have adopted this view entirely overlook my earlier statement of it, to which in some cases their attention had been called.<sup>3</sup> That, by the way. For our present purpose the point is that, as I said in the papers cited, the method of ideas in the *Phaedo* is the conscious and consistent substitution of the ground for the cause—

<sup>1</sup> 99C: τὸν δεύτερον πλοῦν ἐπὶ τῇ αἰτίας ζήτησιν.

<sup>2</sup> *De Platonis Idearum Doctrina* (München, 1884): "Nobis hoc loco animadvertisse satis erit causam sciendi quam dicit Schopenhauerus omnia causarum genera quodammodo complecti . . . notiones latissime patentes notionum minus generalium causae esse videntur, hae deinceps rerum individuarum atque eis cognominum (συνωνύμων). Idem fit in causis teleologicis et in omnibus causarum generibus quae distinxerunt philosophi. Quo fit ut etiam nostra aetate permulti causas sciendi cum aliis causis permulent; qua in re minime falluntur, modo semper in notionibus versentur nec unquam ad res sensibiles descendant. Plato vero, cum causae physicae temporibus illis nondum accurate pervestigatae essent, ipse autem causas teleologicas vel finales ad omnia explicanda adhibere non satis posse sibi visus sit, (Quod fugit Auffarthium [Plat. *Ideen* 78] qui novam Platonis methodum in Phaedone expositam 'eine durch Zweckursachen die Welt erklärende' appellat. Cf. *Phaedo*, p. 99D. Plato vero in Timaeo demum hoc negotium suscepit) totum se ad causas sciendi contulit et notiones (ideas) omnium rerum, h.e. revera omnium praedicatorum causas esse admittit. Id quod logice si interpreteris quodammodo verum esse jam supra monui; aliter si intelligas nihil est nisi doctrina ontologica qua notionem pro ente absoluto ponuntur."

"The Interpretation of the Timaeus," *American Journal of Philology*, IX, 406: "The theory of ideas offers a safe and non-committal position between the two extreme doctrines. For the theory of ideas is logically nothing but the substitution of the ground (causa cognoscendi) for all other conceptions of cause, final or mechanical. And though the language of the doctrine conveys spiritual rather than materialistic suggestions it does not, if carried out with unflinching consistency, commit us either to final or mechanical causation. There is no reason for assuming that Plato ever receded from this position."

<sup>3</sup> Cf. e.g., *Class. Phil.*, V, 514-15. I will mention only one other of several available examples; cf. *Berlin Phil. Woch.*, February 7, 1920, p. 122, on Otto Wichmann: "seine [i.e., Plato's] Ideenlehre bedeute nicht etwa eine neue Weiterklärung sondern stelle den Verzicht auf eine solche dar."

of what Schopenhauer calls the *causa cognoscendi* for the *causa fiendi* and for all other forms of causation.

If this paper were an apology for Plato, I should go on to show that the completeness and consistency of the substitution or translation justifies Plato against the cavils of his critics, and proves that he is really describing a possible procedure of logic, and not a false *à priori* method of the investigation of nature. "I no longer recognize or understand any other kind of cause," Socrates says.<sup>1</sup> And it is quite true that only by translating all other causes into reasons can the confusion of the reason with the cause be redeemed from fallacy. But all that our present purpose requires is to note that, as Schopenhauer and many before him point out, the syllogism is usually described by Aristotle in terms of a similar apparent confusion of the reason with the physical cause.<sup>2</sup> Aristotle, says Schopenhauer, quite naïvely denominates the premises the causes of the conclusion. It would again exceed the scope of this paper to stay to defend Aristotle by dwelling on his distinction between the merely dialectical syllogism and the scientific syllogism whose principles are primary and are, in a sense, the causes of the conclusion. Aristotle when he pleases is well aware of the difference between a "reason" and a cause and expresses it more neatly than English can by opposing *διὰ* with the genitive to *διὰ* with the accusative. The proof that the planets are near *διὰ τοῦ μὴ στίλβειν* he says is not a scientific syllogism of the cause: *οὐ γὰρ διὰ τὸ μὴ στίλβειν ἐγγὺς εἰσιν· ἀλλὰ διὰ τὸ ἐγγὺς εἶναι οὐ στίλβουσιν*.<sup>3</sup>

Themistius is as explicit as any modern. On *An. Post.* I. 2 he explains that in the scientific syllogism both conditions must concur, and the premises must be the causes not only of the conclusion but of the thing proved: *καὶ τὰ λήμματα μὴ τῆς ἐπιφορᾶς εἶναι μόνον ἀλλὰ καὶ τοῦ πράγματος αἷτια τοῦ ἐπιδεικνυμένου*. Similarly Philoponos in *An. Prior.*, Wallies, 35. 21-22. And so Leibnitz:

Mais particulièrement et par excellence on l'appelle Raison, si c'est la cause non seulement de nostre jugement, mais encore de la verité même, ce

<sup>1</sup> *Phaedo* 100—C101A—C.

<sup>2</sup> Cf. *An. Post.* I. 2. 5, *καὶ αἰτίων τοῦ συμπεράσματος*; *Met.* 1013 b 20, *καὶ αἱ ὑποθέσεις τοῦ συμπεράσματος*. So in the definition of the syllogism *An. Prior.* 1. 1. 5, *λέγω δὲ τῶ ταῦτα εἶναι τὸ διὰ ταῦτα συμβαίνειν*.

<sup>3</sup> Cf. *An. Post.* I. 6. 14: *τὸ δὲ διότι ἐπίστασθαι ἔστι τὸ διὰ τοῦ αἰρίου ἐπίστασθαι*.



qu'on appelle aussi Raison à priori, et la cause dans les choses répond à la raison dans les vérités. C'est pourquoi la cause même est souvent appelée raison, et particulièrement la cause finale.<sup>1</sup>

I need not delay to consider with Maier whether Aristotle consistently developed the syllogism from the point of view of its dialectical or its scientific use. All questions that concern merely the general philological interpretation of Plato and Aristotle may be reserved for a later and fuller treatment of the subject.<sup>2</sup> It is enough that the Aristotelian syllogism, like the method of ideas in the *Phaedo*, is on its face a formulation of, a search for, the cause.

But how do we get the syllogism from the formulas of the *Phaedo*? The answer stares us in the face on the pages of Plato. The cause of anything, Plato says, is the presence of the corresponding idea. The cause, that is, of a quality predicated is the presence of the abstract noun corresponding to the predicate. The cause of hot is hotness, of sick is sickness, of odd is oddness. That is the language of the doctrine of ideas. It is a not unnatural mode of expression for Greek idiom.<sup>3</sup> And naïve speakers or philosophers inclined to a realistic view of the concept may adopt it at any time. Schopenhauer gives many examples, not all of them fair to their authors. Spinoza, as Höfding rightly says, makes no difference between ground and cause. And Hauréau, in his history of scholastic philosophy, traces back to neo-Platonism the deplorable tendency of mediaeval mystics and heretics to lapse into Pantheism by treating the order of generality as the order of existence and deducing everything from the bare idea of being. It is the logic of the "*Liber de causis*" with its *res omnes habent essentiam propter ens primum*; of Dante with his *quanto causa est universalior tanto magis habet rationem causae*, and Plotinus and Proclus frequently say or imply that the causal order is the order of generality.<sup>4</sup> As my present subject is logic, not metaphysics, I will not pursue this train of thought further than to point out that, however it may be with the neo-Platonists,

<sup>1</sup> *Nouveaux Essais*, IV, xvii, 3.

<sup>2</sup> Cf. *infra*, p. 16.

<sup>3</sup> Protag. 360c: δι' ὃ δὲ δειλοί εἰσιν, δειλία ὁμολογεῖται. Meno 72c: ἐν γὰρ τι εἶδος ταύτων ἀπασαι ἔχουσι δι' ὃ εἰσὶν ἀρεταί. *Phaedo* 100D: τῷ καλῷ, etc.

<sup>4</sup> Plot. *Enn.* i. 2. 1; vi. 6. 14; vi. 7. 2, where he explicitly transfers to all other things what Aristotle says of the essence of an event as, e.g., an eclipse, being its causal definition.

there is in Plato himself no tendency to deduce Pantheism from the hypostatization of general ideas and a philosophy of being. But Plato goes on to say that we may complicate this extremely simple reasoning by the further principle that the cause of the cause of anything is also the cause of the thing. If the presence of the idea is the cause, so is also the presence of anything that involves the presence of the idea, anything that always introduces and brings with it the idea.<sup>1</sup> If hotness is the cause of hot, so is fire, that always brings with it hotness. If sickness is the cause of sick, so is fever, that always brings with it sickness. If oddness is the cause of odd, so is threeness, which always brings with it oddness.

Now these formulas pass with slight variations into the Aristotelian syllogism:

Heat is in fire  
Fire is in this body  
Heat is in this body

The presence of fire is the presence of heat  
Fire is present with this body  
Heat is present with this body

In modern terms: all fiery things are hot, this body is fiery, this body is hot. Similarly, we may translate

sickness is in fever  
fever is in this man  
sickness is in this man

or

oddness is in the triad  
the triad is in this group of books  
oddness is in this group of books

into

all feverish men are sick  
this man is feverish  
this man is sick

and

all groups of three are odd  
these books are a group of three  
these books are odd

<sup>1</sup> *Phaedo* 104e, ἐπιφέρει; 105a, ἐπιφέρει, etc.; 105a, ἡκεῖ . . . φέρουσα; 105d, ἐπιφέρει; Plotinus *Enn.* I. 1. 2, ἐποιστικόν; IV. 7. 3, ἐπιφέρειν τὴν ἑωήν. Cf. *infra*, 11, n. 3 at p. 12.

Here we have quite clearly and distinctly the principle of the middle term of the syllogism and not merely the word "middle" as in the passage of the *Philebus*.<sup>1</sup> The theory of ideas not infrequently employs the conditional form: if goodness is present a thing is good; if hotness it is hot. A passage of the *Hippias Major* (288a) shows how from this we may derive a hypothetical syllogism or rather a syllogism ἐξ ὑποθέσεως. For example, If symmetry is the idea or definition of beauty, or the self-beautiful, by which all things are beautiful, that with which symmetry is present is beautiful. Symmetry is present with the Parthenon, the Parthenon is beautiful.

It may be objected that I have begged the question by thus translating the Platonic formula into modern terminology. I do not think that the translation, if it be one, imports anything into the Platonic form that is not there. Moreover, though Aristotle sometimes employs the modern form, his language usually much more nearly resembles Plato's than it does ours. In his description of the syllogism he usually employs ὑπάρχει belongs, or κατηγορεῖται is predicated of, instead of forms of the verb to be,<sup>2</sup> and in this language the Platonic syllogism would run,

heat belongs to all fire or is predicated of all fire,  
fire belongs to A or is predicated of all A,  
heat belongs to A or is predicated of all A

which in our terminology yields the syllogism,

all fiery things are hot,  
all suns are fiery things,  
all suns are hot.

Aristotle sometimes approaches still nearer to the Platonic expression, actually employing Plato's words παρῆναι and ἐπιφέρειν.<sup>3</sup>

<sup>1</sup> *Supra*, p. 5.

<sup>2</sup> In *Topics* ii. 1, e.g., he states propositions with εἶναι but combines them for proof with ὑπάρχει. Cf. *infra*, p. 13.

<sup>3</sup> For παρῆναι cf. *Topics* viii. 4. 11. *An. Prior.* I. 28. 3: δὲ δὲ μὴ ὑπάρχειν εἰς ἃ μὴ ἐνδέχεται αὐτῷ παρῆναι. *Ibid.* xviii: πάλιν, εἰ τὸ Β καὶ Η μὴ ἐγχωρεῖ τῷ αὐτῷ παρῆναι. *Met.* 1018 a 23: καὶ ὅσα μὴ ἐνδέχεται ἅμα παρῆναι τῷ ἀμφοῖν δεκτικῷ. For ἐπιφέρειν and συνεπιφέρειν cf. *Topics* vi. 6. 17 and 19: ἐπιφέρει γὰρ ἐκάστη τῶν διαφορῶν τὸ οἰκείον γένος; also viii. 2. 5. The use of ἀκολουθεῖν and ἔπασθαι in *An. Prior.* I. 27 and I. 28 is instructive. Cf. I. 27: ἀνάγκη μὲν γὰρ εἰ ἀνθρώπῳ ἀκολουθεῖ τὸ ζῶον, καὶ τοῦτο αἰσιν ἀκολουθεῖν. Cf. I. 29. 22. There is no syllogism πλὴν διὰ τῶν ἐπομένων καὶ οἷς ἔπεται ἕκαστον.

The Platonic syllogism, it will be noted, is expressed in what mediaeval and modern logicians call intension. Instead of all men are mortal, Socrates is a man, Socrates is mortal, it reads: Mortality is in (present with) humanity, Humanity is in (present with) Socrateity, Mortality is in (present with) Socrateity. There have not been wanting philosophers to defend the syllogism in intension. And many metaphysicians and psychologists regard it as a closer approximation to reality and to the actual process of our thinking. Jevons says:

Every argument involving concrete plural terms might be converted into one involving only abstract singular terms, and vice versa. But there are reasons for believing that the intensive or qualitative form of reasoning is the primary and fundamental one.<sup>1</sup>

This seems to be the view of Taine in the somewhat metaphysical concluding chapters of his *De L'Intelligence* (Paris, 1888), II, 392-93:

Mais homme est un caractère inclu dans Pierre extrait de lui, plus général que lui. . . . D'ou l'on voit que dans le cas des objets individuels soumis à des lois connues l'intermédiaire qui relie à chaque objet la propriété énoncée est un caractère inclus en lui plus abstrait et plus général que lui commun à lui et à d'autres analogues, et qui entraînant par sa présence la propriété énoncée l'importe [ἐπιφέρει] avec lui dans chacun des individus auquel il appartient.

That is, Taine would syllogize: mortality is in or present with humanity and humanity being present with or in Socrates "entraîne" (Plato's ἐπιφέρει)<sup>2</sup> the more general quality of mortality with it.<sup>3</sup> Similarly James writes:

M. is not a concrete. . . . It is an abstract character which may exist embedded with other characters in many concretes. . . . We might conceive of this being a world in which all such general characters were independent of each other so that if anyone of them were found in a subject S we never could be sure what others would be found alongside of them. . . . But the world we live in is not one of this sort. Though many general characters seem indifferent to each other, there remain a number of them which

<sup>1</sup> *Principles of Science*, p. 48.

<sup>2</sup> Cf. p. 256: "Ainsi chez un animal quelconque, la présence des mamelles amène celle des vertèbres."

<sup>3</sup> Cf. p. 407: "A mon avis, c'est dans cet ordre, d'après la compréhension et non d'après l'extension, qu'il faut ranger les termes. De cette façon le raisonnement devient une analyse et non un jeu logique comme le syllogisme ordinaire."



affect constant habits of mutual concomitance and repugnance. . . . They hunt in couples as it were.<sup>1</sup>

The resemblance to the language of the *Phaedo* here is very close. Cf. 103E, 104A, e.g., διὰ τὸ οὕτω πεφυκέναι ὥστε τοῦ περιττοῦ μηδέ ποτε ἀπολείπεσθαι. Similarly Philoponos in *An. Prior. Wallies*, p. 32. 11, inquiring how the soul got the idea of the syllogism, says that it is because things are partly combined and partly disjoined. εἰ δὲ ἦσαν πάντα ἕτερα τὰ πράγματα, οὐκ ἂν τοῦτο ποιῆσαι ἐδύνατο. It would be easy to multiply similar quotations from the modern literature of logic and philosophy. I will conclude with one from the more sober John Stuart Mill:

Though the meaning of all propositions in which general terms are used is in comprehension, writers on logic always explain the rules of the syllogism in reference to extension alone. It is because the formers of the rules did not concern themselves with propositions or reasoning as they exist in thought but only as they are expressed in language.<sup>2</sup>

Mill goes on to explain that this practice of the logicians is on the whole the most convenient way.

The propositions in Extension, being, in this sense, exactly equivalent to the judgments in Comprehension, served quite as well to ground forms of ratiocination upon; and as the validity of the forms was more easily and conveniently shown through the concrete conception of comparing classes of objects than through the abstract one of recognizing coexistence of attributes, logicians were perfectly justified in taking the course which, in any case, the established forms of language would doubtless have forced upon them.<sup>3</sup>

It would then be a short cut to our conclusion to argue that the Aristotelian syllogism, like the Platonic, is a syllogism in intension. But though, as we have said, Aristotle's use of "belongs," "is predicated of," "is present with," and "brings with it," as substitutes for the verb to be points in this direction, it would both complicate and weaken the argument to rest the case on this consideration.

Aristotle usually thought of the terms of the syllogism as in the relation of whole and part.<sup>4</sup> Plato already spoke of the particular as a part of the general idea under which it falls.<sup>5</sup> And Aristotle

<sup>1</sup> *Psychology*, II, 337.

<sup>2</sup> *On Hamilton*, II, 194.

<sup>3</sup> *Ibid.*, p. 195.

<sup>4</sup> *An. Post.* II. 6. 3: ἀεὶ γὰρ ὅλη ἢ μέρος ἢ πρότασις, ἐξ ὧν ὁ συλλογισμός.

<sup>5</sup> *Unity of Plato's Thought*, p. 52.

continues this way of speaking.<sup>1</sup> Now it is possible to conceive the relation of whole and part in intension. Aristotle himself, in *Physics* iv. 3, enumerating the various meanings of *en*, *in*, says that we may use it of the species being in the genus, or the genus in the species; and Themistius' comment on this is one of the few express recognitions in antiquity of the distinction between extension and intension.

Do not be surprised if animal is said to be in man, the more in the less. For if you pause to reflect closely, you will find that animal is in man, as it were as a part.

καὶ μὴ θαυμάσης εἰ τὸ ζῶον ἐν τῷ ἀνθρώπῳ λέγεται, τὸ πλεῖον ἐν τῷ ἐλάττω· εἰ γὰρ ἐπιστήσεις ἀκριβῶς, σχεδὸν ὡς μέρος εὐρήσεις τὸ ζῶον ἐν τῷ ἀνθρώπῳ· ἐν γοῦν τῷ λόγῳ αὐτοῦ περιέχεται.<sup>2</sup>

This use of "in" exactly corresponds to the syllogism in intension which we have found in the *Phaedo*:

Mortality is in animal  
Animal is in man  
Mortality is in man<sup>3</sup>

It would be possible for ingenuity to interpret in this sense the *en* of Aristotle's ἐν ὅλῳ εἶναι and so to attribute to him a consistent formulation of the syllogism in intension.<sup>4</sup> But it would be a mistake. Though, as we have seen, much of the Aristotelian terminology points to intension, he ordinarily thought, as Mill says men ordinarily do, of the exclusions and inclusions of the members of larger and smaller groups of things, that is, in terms of extension. To that extent, those interpreters are right who insist that the Aristotelian logic is a

<sup>1</sup> Cf. *Met.* 1022 a 33: τοῦ γὰρ ἀνθρώπου πολλὰ αἷτια τὸ ζῶον τὸ διπλόν. 1013 B 33: καὶ αἰεὶ τὰ περιέχοντα ὁτιοῦν τῶν καθ' ἑκαστα. *An. Post.* I. 31. 6: τὸ δὲ καθόλου τίμιον, ὅτι δηλοῖ τὸ αἷτιον. *An. Prior.* I. 4. 1: ὅλως γὰρ δὲ μὴ ἔστιν ὡς ὅλον πρὸς μέρος καὶ ἄλλο πρὸς τοῦτο ὡς μέρος πρὸς ὅλον, ἐξ οὐδενὸς τῶν τοιοῦτων δεικνυσιν δὲ δεικνύων οὐδὲ γίγνεται συλλογισμός.

<sup>2</sup> Cf. *Ar. de Interp.* xi. 7: ἐνυπάρχει γὰρ ἐν τῷ ἀνθρώπῳ τὸ ζῶον.

<sup>3</sup> Cf. *Phaedo* 105b; *infra*, p. 16.

<sup>4</sup> It is possible that this was Leibnitz' idea when in *Nouveaux Essais*, IV, 17, 8, he writes: "en effect le prédicat est dans le sujet, ou bien l'idée du prédicat est enveloppée dans l'idée du sujet. . . . La manière d'enoncer vulgaire regarde plustost les individus, mais celle d'Aristote a plus d'égard aux idées ou universaux." On this Fonsegrove (*Annales de la Faculté des Lettres de Bordeaux*, III, 396) comments: "Cela revient à dire qu'Aristote envisage la proposition du point de vue de la compréhension, tandis que les logiciens l'envisagent plutôt de celui de l'extension." *Ibid.*, p. 400, Fonsegrove argues against Hamilton and with Mill that Aristotle formulates the *syllogism*, as opposed to the *proposition*, in extension.

logic of extension.<sup>1</sup> But philologically, the matter is much more complicated than they perceive, and the interpretation of all the texts that bear on *θέσις* or the position of the terms in each of the three figures and the meaning of *ἐν ὅλῳ εἶναι* would be a very delicate and laborious task.<sup>2</sup> It is not necessary for our purpose. Our abandonment of the attempt to force consistency upon Aristotle in this matter precludes us, it is true, from the direct and peremptory argument that the Aristotelian syllogism, being in intension, must therefore be directly derived from the Platonic form. But it does not in the least tend to disprove that derivation when other considerations render it probable. For Aristotle himself was not clearly conscious of the distinction, which comes to us from the mediaeval Schoolmen. And it is very easy to pass unconsciously, as he sometimes seems to do, from the one form to the other. How easy it is appears from the comment attached in Diogenes *Laertius* viii. 60, to a Stoic definition of genus: *γένος δέ ἐστι πλειόνων καὶ ἀναφαιρέτων ἐννοημάτων σύλληψις, οἷον ζῶον. τοῦτο γὰρ περιείληφε τὰ κατὰ μέρος ζῶα*. The last clause is perhaps a misapprehension of the definition, which seems to be in intension—the genus is the unity of the concepts of the species inseparable in it. This would explain *ἀναφαιρέτων*, which some editors have suspected. Plato himself has many syllogisms in extension and anticipates the Aristotelian language of extension when in the *Euthyphro*<sup>3</sup> he explains the impossibility of the direct conversion of the universal affirmative.

What more, then, is needed to establish our conclusion? We have seen the improbability of all alternative theories and answered

<sup>1</sup> E.g., Minto, pp. 47, 67, 171.

<sup>2</sup> In *An. Prior.* I. 1. 24 b 26 Aristotle says: *τὸ δ' ἐν ὅλῳ εἶναι ἕτερον ἑτέρῳ καὶ τὸ κατὰ παντὸς κατηγορεῖσθαι θάτερον θατέρου, ταυτὸν ἐστι*. This is true only if the reference of the terms *ἕτερον* . . . *θάτερον* is reversed in the two clauses and *ἐν ὅλῳ εἶναι* does not mean as sometimes taken "in the whole of it," but as Maier and Ross after him rightly construe "in it as (a part is) in a whole." Grote's "the subject is or is not in the whole predicate and Julius Pacius' "Subjectum dicitur esse vel non esse in toto attributo" are ambiguous or wrong. Philoponus, *Wallies*, 39. 5 says: *λέγομεν ἐν ὅλῳ εἶναι τὸν ἀνθρώπον τῷ ζῳῳ ἀντὶ τοῦ ὑπὸ δλον αὐτὸν περιέχεσθαι τοῦ ζῳου*.

<sup>3</sup> 12BC: *ἀλλ' ἵνα μὲν αἰδῶς ἔνθα καὶ θεός, οὐ μέντοι ἵνα γε θεός πανταχοῦ αἰδῶς ἐπὶ πλεον γὰρ οἶμαι θεός αἰδῶς*. Cf. *Ar. An. Post.* II. 14 or 12: *ἀλλὰ καὶ τὸ περιττὸν ὑπάρχει τε πάσῃ τριάδι καὶ ἐπὶ πλεον ὑπάρχει, καὶ γὰρ τῇ πεντάδι ὑπάρχει*, which also serves to illustrate *Phaedo* 104a. Cf. also *Topics* iv. i. 10: *στοιχείον δὲ πρὸς ἅπαντα τὰ τοιαῦτα τὸ ἐπὶ πλεον τὸ γένος ἢ τὸ εἶδος*.

all the more plausible objections to our own. We have shown that the *Phaedo* passage not only easily yields the formula of the syllogism but embodies its principle: the discovery and use of the middle term. For that which always brings with it the predicate or quality which we desire to prove is plainly the Aristotelian middle term. *Ar. An. Prior. I. 1. 25 b 35*: καλῶ δὲ μέσον μὲν ὁ καὶ αὐτὸ ἐν ἄλλῳ καὶ ἄλλο ἐν τούτῳ ἐστίν. *Phaedo* 105B: εἰ γὰρ ἕροιοῦ με  $\phi$  ἂν τι ἐν τῷ σώματι ἐγγένηται θερμὸν ἔσται, οὐ τὴν ἀσφαλῆ σοι ἐρῶ ἀποκρισιν ὅτι  $\phi$  ἂν θερμότης, ἀλλὰ κομψοτέραν ἐκ τῶν νῦν ὅτι  $\phi$  ἂν πῦρ. Clearly, the fire, like Aristotle's middle term, is itself in something and something is in it, and when it has done its office it disappears in the conclusion of the Platonic syllogism as in the Aristotelian. Finally, we have shown that the Platonic syllogism, like the Aristotelian, is the discovery or statement of a cause, and that though Aristotle did not consciously and normally conceive the syllogism in intension, many of his expressions are best understood by reference to its formulation in this way by Plato. For the submission of the theory to the judgment of scholars, that suffices.

Two things might be added which would convert this article into the book for the sake of which I have delayed its publication so long, and which I may yet try to write. There could be added first a philological interpretation of all passages in Aristotle that bear remotely on the question,<sup>1</sup> and likewise of *Phaedo* 96-107, with examination of the conflicting views of critics ancient and modern. This would not really strengthen the argument, and it would make it unintelligible except to a few Aristotelian and Platonic specialists who probably would not see the wood for the trees. But it would be the approved philological procedure. The other amplification would consist of illustrations that would lend the simple formula at which Plato himself smiles a little more dignity in modern eyes.

I will conclude with a few words on this point. Suppose we reason: the presence of crystallization is the presence of double refraction; crystallization is present with Iceland spar, etc.; or the presence of mountain valley air is not the cause of endemic goitre, because goitre is frequent in Chicago, where mountain air is not

<sup>1</sup> Cf. *supra*, pp. 9 and 15.



present;<sup>1</sup> is it not plain that we can if we choose fit our reasoning to the Platonic formula?

I trust this will not be mistaken for the claim that Plato anticipated modern science, or that his syllogism, if syllogism it be, is practically helpful in the laboratory. I am merely illustrating the fact that the Platonic formula is the syllogism so far as the syllogism can be used, not for investigation, but in the logical description of investigation. If, for example, instead of the seeming tautology: the presence of fever is the presence of sickness, etc., we reason: the presence of typhoid is the presence of fever, the presence of *bacillum Pythosum* is the presence of typhoid, is it not obvious that the Platonic formula describes the method of Pasteur and of the science of bacteriology as clearly as the syllogism can and somewhat more vividly? And as a matter of fact there is, by way of Bacon, a direct historical connection between the Platonic syllogism and the logic of modern inductive science. Once more let me enter a caveat against the imputation of maintaining that the method of Bacon is the method of the modern laboratory. I merely wish to point out that in so far as the method of Bacon or that method improved by Mill can be used to describe the procedure of inductive science it is again the Platonic logic of presence and absence. For the Baconian doctrine of Forms is merely the formula of the *Phaedo* applied to the systematic search for the unknown bearers or bringers of qualities or effects that nearly concern us. Bacon explicitly commends the Socratic quest for definitions in the Platonic dialogues as the best available illustration of his inductive method though in *aliena materia*:

But the induction which is to be available for the discovery and demonstration of sciences and arts, must analyze nature by proper rejections and

<sup>1</sup> The *Phaedo* passage, not needing the negative for Plato's purpose, does not explicitly add that as that which always brings the quality with it is the cause, so that which does not always bring the quality is not the cause. But we may credit Plato with the capacity for that simple inference. In fact he employs it explicitly in *Republic* 329a: "they allege a false cause, for if old age were the cause, I and other old men would have suffered this thing, etc. But we do not." This is only the logic of common sense, and is found in *Isoc.* xv. 230. It is from this that Aristotle derives his discussion of *μη παρὰ τοῦτο*. *An. Prior.* II. 19: τὸ γὰρ τὸ ἀναλτιον ὡς αἰτιον τιθέναι τοῦτο ἐστιν. For the rest, Plato is in good company in taking the negative half of the statement for granted. Aristotle himself, after defining the middle term positively, as that which is in another and another in it, goes on to recognize the form *ἐν ὅλῳ τῷ πρώτῳ μὴ εἶναι*. And Professor Minto in a textbook of logic actually says that the *Dictum de Omni et Nullo* is: "Whatever is predicated of all or none of a term, is predicated of whatever is contained in that term."

exclusions; and then, after a sufficient number of negatives, come to a conclusion on the affirmative instances: which has not yet been done or even attempted save only by Plato, who does indeed employ this form of induction to a certain extent for the purpose of discussing definitions and ideas.<sup>1</sup>

He proposes to transfer this method to the investigation of the Forms, as he calls them, real physical qualities and events in nature. The Form of a quality as whiteness or heat is that whose presence or absence involves the presence or absence of the quality; and our object in finding the Form is to learn how to produce the quality or effect at will. Here Bacon may seem to diverge fundamentally from the thought of the ancients. For the Philistine Xenophon represents Socrates as contemptuously asking the physicists whether if they learn the causes of winds and weathers they expect to be able to produce them at will.<sup>2</sup> But the Platonic Socrates in the *Laches* (189e) speaks almost in the very words of Bacon:

If we know about anything that its presence with a thing makes that thing better, and if we are further able to bring about its presence with that thing, it is obvious that we (must) know the thing itself.

εἰ γὰρ τυγχάνομεν ἐπιστάμενοι ὅπου οὖν περὶ ὅτι παραγενόμενον τῷ βέλτιον ποιῇ ἐκεῖνο ὃ παρεγένετο, καὶ προσέτι οἱοί τε ἔσμεν αὐτὸ ποιῆν παραγίγνεσθαι ἐκείνῳ, ὁῦλον ὅτι αὐτὸ γέ ἴσμεν τοῦτο οὐ περὶ σύμβουλοι ἂν γανοίμεθα ὡς ἂν τις αὐτὸ ῥᾶστα καὶ ἄριστ' ἂν κήσαιοτο.

There is a suggestion of the idea in Cic. *Tusc.* i. 10: "Nam ut medici causa morbi inventa curationem esse inventam putant, sic nos causa aegritudinis reperta medendi facultatem reperiemus." Themistius *An. Post.* i. 17 has Bacon's "convertible with the given nature," *Nov. Org.* ii. 4 (ἀντιστρέφει) and his "muster of all known instances which agree in the same nature," *Nov. Org.* ii. 11 (δεῖ οὖν ζητεῖν ὁ κοινῇ πᾶσιν, etc.). Cf. also Themistius on *An. Prior.* i. 5. A little search would doubtless discover other anticipations of the thought in post-Aristotelian philosophy. This is the underlying idea of the first four aphorisms of the *Novum Organum* and particularly of the fourth:

For a true and perfect rule of operation . . . is the same thing with the discovery of the true Form. For the Form of a nature is such that given the Form the nature infallibly follows.<sup>3</sup> Therefore it is always present when the nature is present and . . . if it be taken away the nature infallibly van-

<sup>1</sup> *Novum Organon*, I. CV.

<sup>2</sup> *Mem.* i. 1. 15.

<sup>3</sup> Cf. *ἔπεραι*, *supra*, p. 11, n. 3.

ishes. . . . Lastly the true Form is such that it deduces the given nature from some source of being which is inherent in more natures, and which is better known in the natural order of things than the form itself.<sup>1</sup>

When Bacon hints that in the last analysis the Form may prove to be only the latent arrangement and movements of unseen particles he combines this Platonic logic with Democritean physics.

Even when the doctrine is thus interpreted the working scientific man may ask with Persius' *Centurion*: "Cur quis non prandeat hoc est?" His problem is not to prate of logic but to devise experiments that will eliminate the things whose presence does not bring in (*εἰσφέρειν*) the quality and isolate that whose presence involves its presence and whose absence involves its absence, and which is thus the sole invariable antecedent. So Aristotle in chapter xxx of *An. Prior.* ii rests the possibility of a science of physiognomy on the application of this method: *εἰ τοίνυν ταῦτα ἐστὶ, καὶ δυνησόμεθα τοιαῦτα σημεῖα συλλέξαι ἐπὶ τούτων τῶν ζώων, ἃ μόνον ἐν πάθος ἔχει τί ἴδιον . . . δυνησόμεθα φυσιογνωμονεῖν.* There are other passages in Aristotle, and still more in Themistius, that distinctly anticipate the canons of inductive logic for isolating the cause regarded as the sole and invariable antecedent.<sup>2</sup>

I cheerfully concur in the opinion that mother-wit and acquaintance with the facts will serve the working scientific man in this task far better than the study of Plato and Aristotle, even if supplemented with a reading of the Aristotelian commentators. But for those who still retain any interest in the history of ideas and the true mind in the making few pages in European literature can match the significance of those few pages of the *Phaedo* which have so often been dismissed as sophistical or mystical verbiage but which prove to be the source at once of the Aristotelian syllogism and of Baconian induction.

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<sup>1</sup> Cf. *supra*, pp. 10-11 and 12.

<sup>2</sup> Cf. *An. Post.* II. 12. 22.

## THE ROMAN CALENDAR AND THE REGIFUGIUM

By ELMER TRUESDELL MERRILL

"Every schoolboy knows" that our civil calendar is substantially that established by Julius Caesar in 46 B.C. as a modification of the more ancient Roman calendar of Republican days; but that we follow for the names of two of the months, July and August (instead of Quinctilis and Sextilis), the use of the period shortly after that of the great Julius, and furthermore that we very sensibly and conveniently designate the days in each month by numbering them in a single consecutive series forward, as they actually progress, instead of numbering them backward in three series from the fixed points of Kalends, Nones, and Ides, as the Romans did.

All this the schoolboy can readily comprehend, and even in some degree approve. He can also tolerate our habit of adding the extra day of leap-year to the month of February, though he may sometimes wonder (and with good reason) why we do not put it rather at the end of the year, in December, where it would seem rationally to belong, and would make a desirable addition to the length of the Christmas holidays.

But after he has mastered in the course of his Latin studies the troublesome method of Kalends-Nones-Ides dating, he finds yet another grievance against the Roman calendar. The Romans are evidently responsible for our putting of the extra day of leap-year in the month of February, for they did it themselves; and the reason was that at the very early date when the practice was first introduced of using intercalation to make the calendar square with the sun, February was indeed the last month of the year and March the first, as the numerical designation of the months of September, October, November, and December still continues to indicate, as also Quinctilis and Sextilis once did. In Caesar's time, and for a century before, the year was commonly reckoned for many civil purposes as beginning with January, as it does at present, but Caesar did not venture to go so far as to depose February from its immensely ancient position as

the leap-year month, and we have not yet dared to depart from the following of Caesar in that regard.

But the schoolboy has to learn that Caesar did not make his leap-year by simply adding one day at the end of February, as we appear to do.<sup>1</sup> He managed it instead by assigning the twenty-fourth of February to two successive days; and as the twenty-fourth of February was *a.d. VI Kal. Mart.*, the leap-year was later called "bissex-tile." Appeal by the boy to his schoolmaster anent this seemingly wilful perversity of the Caesarian mind is likely to bring the answer that Caesar played this trick with *VI Kal. Mart.* simply because the Romans before him had for untold centuries made their intercalations there, and not after February 28.

But that answer simply removes the natural question one step farther back, though it is indeed a long step, and carries the matter far behind classical times and into the region of hoary antiquity. Why did those primitive Romans fix upon that place instead of February 28 as the key-date for the leap-year intercalation? To that question the master probably can give no answer. He is pardonable; practically all our extant ancient authorities appear to have been just as ignorant on that point. Nor (so far as my knowledge goes) could the avid curiosity of the boy find any satisfaction in his schoolbooks of reference, or even in the works of many learned scholars and specialists. Most of these imitate the ancients in making no reference at all to the reason why *VI Kal. Mart.* is thus distinguished. So very eminent an authority as Professor J. S. Reid says, "Why the *mensis intercalaris* was not placed in what seems its natural position, after the last day of *Februarius*, has not been satisfactorily explained."<sup>2</sup> He does not say, however, that no essays have been made in that direction, though he proffers none himself.

<sup>1</sup> But the ecclesiastical calendar of the Roman Catholic Church (I do not know about the Orthodox Churches of the East) still recognizes Caesar's determination; for the day of St. Matthias, which in normal years falls on February 24, is on leap-years transferred to February 25, of course on the theory that the day which in leap-years follows February 23 is the intercalated day. The Churches of the Anglican Communion provide in their calendar for no such transfer of St. Matthias' Day. Whether the transfer survived the Reformation in England even for a season, or ever was the rule there, I do not know. No mention of any such transfer is made in the First Prayer-Book of Edward VI (1549), or in succeeding service-books.

<sup>2</sup> In *A Companion to Latin Studies*<sup>3</sup>, p. 94, § 109.

The obvious reply to the boy's natural question would of course appear to be that at the time the periodic correction of the calendar by such intercalation was introduced, February must have had only twenty-three days and not twenty-eight. Hence the days necessary to bring the calendar year into agreement with the solar year, and thus to preserve the seasonal dates for the recurrence of the festivals of seedtime and harvest, and all the other celebrations that had a proper relation to the seasons were naturally added in this place at the end of the year.

Yet this answer is not so simple as it looks. It involves difficulty. But it has been set forth by one man, the much-criticized Huschke.<sup>1</sup> He followed certain ancient authority (which will be mentioned later) in believing that in very ancient times the day of the Terminalia (February 23 in the later calendar) was the very last day of the Roman year: therefore, since the intercalations, whether of one day (as once every four years into Caesar's normal 365-day year) or of twenty-two or twenty-three days alternately in alternate years (as into the normal 355-day year of the centuries preceding Caesar's reform), were definitely asserted by the ancients to have been made immediately after the Terminalia (February 23) and before the Regifugium (February 24),<sup>2</sup> these leap-year intercalations actually were introduced in the beginning of such things in their natural place at the end of the year.

The occurrence of the Regifugium immediately after the Terminalia made no trouble for Huschke in reaching the conclusion that the earlier festival stood at the very end of the year, since he believed, with most of the ancients, that the Regifugium commemorated the driving out of King Tarquin, and hence he thought it was of much

<sup>1</sup> Ph. E. Huschke, *Das Alte Römische Jahr und Seine Tage* (Breslau, 1869); v. Register s.v. Terminalia.

<sup>2</sup> Varr. *L. L.* VI. 13 *quom intercalatur, inferiores quinque dies duodecimo demuntur mense: Cens. Die Nat.* 10 [Caesar] *instituit ut peracto quadrienni circuitu dies unus, ubi mensis quondam solebat, post Terminalia intercalaretur; 20. 6 in mense potissimum Februario inter Terminalia et Regifugium intercalatum est: Macrob. Sat. I. 13. 15 Romani non confecto Februario sed post uicesimum et tertium eius diem intercalabant, Terminalibus scilicet iam peractis.*—It may be remarked that *potissimum* in the passage from Censorinus does not imply, as some moderns have taken it, that the intercalation was sometimes made in another month than February, but that it was always made in February rather than elsewhere.



later introduction into the calendar than the Terminalia. On this matter something will be said later. He appears to have thought the Equirria also (February 27) to be of later origin.

Huschke's heretical opinion did not succeed in establishing itself in public favor. Otto Ernst Hartmann,<sup>1</sup> who subjected Huschke to much contradiction on other points also, objected to this that a 23-day month stood in no possible integral relation to a lunar year, and that none of the ancient authorities speak of February as ever having been a month of that content. Later writers appear to have thought Huschke's theory simply unworthy of notice—but I have not succeeded in reading all of the articles and pamphlets on Roman chronology that have been poured forth in such profusion in the years since Huschke's time. A pretty full list of them up to 1889 is contained in Soltau's book, which is about to be mentioned. Later ones can readily be found by reference to the *Bibliotheca Philologica Classica*.

Twenty years after Huschke's book was issued, Wilhelm Soltau put forth another solution of the question why the intercalary month was inserted immediately after the Terminalia rather than after February 28.<sup>2</sup> To him his explanation was both simple and decisive.<sup>3</sup> He began it by pointing out that according to Roman feeling any intercalation must be so made as to disturb as little as possible the traditional enumeration of the normal series of days. An intercalation made after the last day of February (the twenty-eighth) would disturb the numbering of a full half of the month, since all the days later than the Ides (February 13) would have to be dated from the intercalary Kalends, instead of, as normally, from the Kalends of March. An intercalation after the Ides of February would, to be sure, work no confusion at all; but it was not made there because the sacred days of February form a connected whole, and therefore, according to Roman feeling, must not thus rudely be severed by an

<sup>1</sup> In his *Der Römische Kalender* (Leipzig, 1882). But Hartmann died in 1871, and his book was edited and issued by Ludwig Lange.

<sup>2</sup> In his *Römische Chronologie* (Freiburg, 1889), pp. 39 ff.

<sup>3</sup> "Der Grund ist einfach. . . . Durch die hier gegebenen Ausführungen ist auch die so oft ventilirte Frage nach der Existenz, ja nach der Stellung eines *dies intercalaris* entschieden."

interjected month. The Terminalia was the great festival of the closing year (*Jahresschlussfest*),<sup>1</sup> so the intercalation was made immediately after that feast-day, and the Romans simply put up of necessity with the incongruity that on leap-years the festivals of Lupercalia (February 15) and Quirinalia (February 17) lost their old numerical designations, and had to be dated from the *Kalendae intercalares*; but on this point the pontifical officials tried to help themselves out somewhat by dating the days of February between the Ides and Terminalia each as such a day before the latter (for example, not *a.d. X Kal. Interc.*, but *a.d. IX Terminalia*); that this mode of dating was actually sometimes used is made clear by Soltau's citation of two examples of it.

The explanation does not appear to be so simple and conclusive as its author thought it. He starts with the assumption of a certain attitude of the Roman mind. He does not attempt to establish its actual existence by other examples. Yet it is far from appearing to be self-evident. Its existence requires further substantiation. I do not think the author could have found any that would be satisfactory.

This notion of Soltau's might seem to be founded eventually on such passages as Cens. *Die Nat.* 20. 9 and Macrob. *Sat.* I. 14. 8, where these writers explain that Caesar, in adding to the length of certain of the months, appended the new days to the end of the month in order not to disturb the position of the sacred days within the month itself (of course counting from the beginning of the month in each case). But that is to invent a hieratic reason for what was simply in ultimate origin a natural act, and at any rate the alleged fact does not strengthen Soltau's theory, and he does not quote Censorinus and Macrobius in his own support.

But that first assumption of Soltau's would prove too much; it would indicate that the intercalation should be made immediately

<sup>1</sup> This is by no means to be taken as indicating that Soltau followed Huschke in believing Terminalia to have been actually at one time the last day of the year, or to have had any connection with other than land-boundaries. In *Jahresschlussfest* Soltau probably was interpreting (in his own fashion) Ov. *Fast.* II. 50 *tu quoque sacrorum, Termine, finis eras*. Of course Ovid knew well enough that Regifugium and Equirria came in the February days following Terminalia. But Soltau would avoid the notion that Terminalia ever was actually the last day of the year; therefore he apparently believes Ovid to mean that Terminalia was the festival of the end of the year though not coming precisely at the end of the year.

after the Ides of February, which was not done in fact. Therefore the author introduces yet another assumption of the subjective mind, in order to justify the placing of the intercalation after Terminalia. But this in turn in the case of two festivals brings the enumeration into conflict with the first principle already assumed. So, driven to desperation, he says that the Romans simply put up with these two anomalies; though he afterward adds that they helped themselves out by the system of dating from the Terminalia. He speaks of this as though it were a recognized system, introduced especially for the specified purpose. But, on the other hand, if a man were at a distance from Rome and wished to set down the date (between the Ides of February and Terminalia), but by reason of his absence from the capital did not know whether or not an intercalary month had been proclaimed,<sup>1</sup> he could save himself by dating *ante Terminalia*. That, I think, is likely to be the true explanation of the two instances of the sort that alone Soltau could quote. But I might add that I think it likely that such a system of popular (not in origin merely pontifical) dating from the great festivals was of quite primitive character, not at all devised for the purpose Soltau intimates, and that it continued to be in popular use perhaps much more fully than we might be inclined to think.<sup>2</sup>

If the intercalation had been made at the end of the month, only five more days (February 24-28) would have suffered a change in their normal enumeration (fifteen days instead of ten only), and inconsistency in the treatment of the days of February after the Ides

<sup>1</sup> Such proclamation was probably made on the Nones of February (February 5), when the *rex sacrorum*, and of course before him the King, announced the festivals of the month (the Poplifugia of July 5 is the only primitive festival assigned to a day between Kalends and Nones of any month): cf. Varr. *L. L.* VI. 13 *rex cum ferias menstruas Nonis Februariis edicit*. Quirinalia and Lupercalia would have a different designation in leap-years from that in normal years, and hence the announcement of the approaching intercalation would appear to be almost necessarily connected with the proclamation of the February festivals.

<sup>2</sup> Especially I believe the dating backward from Terminalia to be of very antique origin. The years were not at that time regarded as if they proceeded in an endless chain, month after month indefinitely. Each was taken as complete in itself. The year ended definitely with Terminalia, or with the intercalation that followed it, when one was made. The new year, beginning on the Kalends of March, would be, like the old one, a distinct entity. Therefore the days of February after the Ides could not properly be dated from the first day of the new year, but must be from the last day of that which was passing away.

would have been avoided. Is it likely that, since they must make some sacrifice of principle, according to Soltau's theory, in the case of ten days, the Romans would have thought it more convenient or proper to put up with this incongruity of treatment for the sake of preserving the integrity of only that group of the last five days of the month?

It should of course be noted that Soltau limits the inconsistency in the application of his first principle to only two days, and those festivals, the Lupercalia (February 15) and Quirinalia (February 17), whereas the truth is that all the days after the Ides (February 14-23) in leap-years suffered change of designation, and these ten days included the great festival of the Terminalia itself. This is a serious defect in Soltau's case. There were ten exceptions to his rule, and not two only, and one of these ten was of a great festival that he disregards.

Soltau further leaves the Regifugium and Equirria out in the cold altogether. He does not mention them. Yet they are, according to his theory, equally February festivals with the others, and the principle of non-separation of February sacred days would appear properly to apply to them as much as to the rest. If Terminalia is merely the festival of the approaching end of the year, and not the final festival of the year on actually the last day of the year, then the sad lot of the excluded Regifugium and Equirria ought to be provided for in some way by any theory that claims to be simple and decisive.

Soltau's explanation has much the manner of the complicated prescriptions that old-fashioned physicians used to compound, in which one ingredient was exhibited to attack the disease, a second to correct some bad tendencies of the first, a third to counteract some infelicities of the second, and so on *ad libitum*. Less involution, less contradiction, less postulation of unsubstantiated states of mind, and more dependence upon ancient sources, appear to be the prerequisites for a satisfactory solution of the problem.

We may now turn our attention to certain matters that concern the Terminalia.

This festival is assigned by the Calendars and other ancient sources to *a.d. VII Kal. Mart.* (February 23). The name is given

in the Calendars in the large letters which indicate that the Terminalia belonged among the most ancient sacred days of the Roman people. The festival is more than once said in the ancient sources to be in honor of the god Terminus, who was regarded as the deity who had in charge the boundaries of real estate, whether public domain or private property. The details of the rites also with which Terminalia was celebrated<sup>1</sup> appear to look upon the god as presiding only over *finis imperi et agrorum*. All the extant ancient glossaries tend in the same direction.<sup>2</sup> Almost all of the ancient authors thus agree.

But one very powerful voice is raised against this popular limited understanding. Varro affirms distinctly that the Terminalia was given that name (he is apparently referring to a far-distant past) because it was assigned to the last day of the year; and, apparently in substantiation of this statement, he says that in leap-years the last five days are subtracted from the month of February (here he is speaking of the custom of his own day; the change of tense is significant).<sup>3</sup>

It seems to be generally believed that the statement of Varro about the Terminalia having been in early times the last day of the year is supported also by Ovid.<sup>4</sup> He is understood to mean that as February was in ancient times the last month of the year, so the Terminalia was the last festival of the year, and was celebrated on the last day of the year. He certainly means that the Terminalia was once the last festival of the year. As he knew that Terminalia was immediately followed in his own time by Regifugium, and Regifugium after an interval of two days by Equirria, it would certainly appear to follow that he must have meant his readers to understand that Terminalia, the last of the yearly festivals, fell also on what was once the last day of the year. But he does not say as distinctly as Varro did that the name of the festival was given from its position at the

<sup>1</sup> V. Ouid. *Fasti* II. 639 ff., and less fully Dionys. *Ant. Rom.* II. 74.

<sup>2</sup> See indexes to the Goetz-Schoell *Corpus*, s.v. Terminalia, Terminus.

<sup>3</sup> Varr. *L. L.* VI. 13 *Terminalia* [sc. dicta sunt], quod is dies anni extremus constitutus: duodecimus enim mensis fuit Februarius, et cum intercalatur, inferiores quinque dies duodecimo demuntur mense (Macrobius will later tell us what was done with them).

<sup>4</sup> Ouid. *Fasti* II. 49 f.

Qui [sc. mensis] sequitur Ianum, veteris fuit ultimus anni:  
Tu quoque sacrorum, Termine, finis eras.

very end of the year. Instead of this he goes on to describe it in usual form as the feast of the god Terminus, and, in all his ensuing discourse and description of the rites with which the country people kept the feast, Terminus is always treated as the guardian of the *fines agrorum*, and never once as having anything at all to do with the *finis anni*.

Ovid's source in general for such antiquarian lore appears to be Verrius Flaccus.<sup>1</sup> If the fates had been more propitious, we might have been able to determine whether Verrius actually went as far as Varro. But not merely is the work of Verrius lost; the pertinent pages of the abbreviation of Verrius by Festus have also vanished, and we are left for Terminus and the Terminalia merely with the epitome of Festus by Paulus. And Paulus has a way (where we can compare him with Festus in the extant parts of the latter's work) of abbreviating by setting down only the first part of the paragraph in Festus and leaving out the rest. In this first part Festus often enough records the popular definition of the word cited, and then goes on with some critical discussion, of course following Verrius therein. This latter part would tend to disappear in Paulus. That may have been the fate of Terminus and Terminalia in the treatment by Festus. Paulus merely tells us, so far as the matter goes that concerns us here, that Terminus had charge of the *fines agrorum*.<sup>2</sup> So we do not know what more Festus and Verrius Flaccus may have said on this point. Verrius probably said no less than Ovid, and so may probably be ranked with Varro as believing that Terminalia was once the last day of the year.

St. Augustine seems to have had some source (Varro?) for his discourse on Janus as the god of beginnings and Terminus as the god of endings.<sup>3</sup> He not merely does not limit Terminus, as most of the ancients appear to do, to the business of *fines imperi et agrorum*,

<sup>1</sup> But if Ovid's main source for such matters of the calendar was, as some critics believe, Varro, then our two witnesses might be reduced to one, but that one a Varro! If Ovid followed Verrius on this point, and Verrius depended for it on Varro, we should similarly be reduced to a single witness, but that a trustworthy one.

<sup>2</sup> Paul., p. 368 (Mueller), 560 (Ponor), 505 (Lindsay) *Termino sacra faciebant, quod in eius tutela fines agrorum esse putabant*.

<sup>3</sup> Aug. *Ciu. Dei* VII. 7; e.g., *in istis duobus diis initia rerum temporalium finesque tractantur*.



but on the contrary he says nothing at all about him as concerned with anything else than the *fines anni*. But it is doubtful whether St. Augustine really means to affirm that the Terminalia in antique times fell on the last day of the year, and not merely that the festival was meant to mark as such the last month of the year. He even says the month was sacred to Terminus, as the preceding month was to Janus. Yet it is of some interest to be able to cite an ancient author who at least appears to assume it as a generally known fact that Terminus had to do with all endings of things, as Janus with all beginnings. Terminus was not simply a god of merestones.

We have thus very clear and positive ancient testimony that Terminalia was once the last day of the year. It comes from witnesses of the highest authority, if we may indeed, as appears probable, join Verrius Flaccus to Varro. Their utterances would be favorably received on almost any other point of Roman antiquities, where it concerned a mere fact. They are not contradicted by any other of the ancients. These others merely say nothing about the matter. The only possible weight that their silence could have in influencing our judgment is due to the fact that they speak of Terminus, when they mention him at all, only as a god of land-boundaries. But it is natural that they should do this. For several centuries before their time the Terminalia had ceased to mark the last day of the year. There appears to have been in existence no especial ceremony that concerned distinctly that as the final day. Therefore, people forgot almost completely that Terminus ever did have anything to do with time-limits as well as with land-limits. They even invented an aetiological story of rivalry to account for the presence before their eyes of a monument representing Terminus in the very temple of Jupiter Capitolinus. And this was in spite of an existing vague memory of a Jupiter-Terminus, of which some traces are handed down even to our times. That ought to have marked Jupiter in his aspect of Terminus as a god of endings in general, precisely as in his aspect of Janus he was a god of beginnings. St. Augustine, indeed, had some notion of this; see his passage just cited (*Ciu. Dei* VII. 7) and the adjacent chapters. He felt that Jupiter-Janus and Terminus ought to have been treated as one god, presiding over both the beginning and the ending of things. Doubtless he was influenced by

the thought of the One God who declared himself to be "Alpha and Omega, the beginning and the ending."

But though people forgot Terminus as a god of all endings, they remembered him constantly as a god who guarded the limits of their fields, because the integrity of their farm-boundaries was a matter of much concern to them, and the relation of the god to these was kept in mind by the welcome annual recurrence of a peasant festival on the day of the Terminalia. To the question why this joyous occasion came in the month of February—the Lenten season of the Romans—they of course never gave a thought.

It was perfectly natural, then, that most of the ancient writers, living at a time centuries after Terminalia had ceased to be the last day of the year, should not speak of Terminus as the god of the year's end, but only of the land's end. Their silence on his other aspect weighs not a grain against the positive testimony already cited.

It is not too much to affirm, therefore, that the ancient testimony is entirely in favor of the proposition that Terminalia was at a very early time the last day of the year. This time must of course have antedated the permanent addition to the year of five more days at the end of February, and the beginning of the correction of the annual calendar by intercalations.<sup>1</sup> The place of the intercalations, known to us as it existed through the centuries, would accordingly appear to be no longer a curious, but a most simple and natural, thing. The intercalations were added to the original end of the year, as we should a priori expect would be the case. Nothing could be more simple and straightforward; nothing else appears to settle the problem which we have been considering.

But this is all on the supposition that we are content to rest back merely on the known state of the actual historic calendar in regard to intercalations, and on the ancient testimony. But difficulties are raised, and some at least of these must be here considered.

Mr. Warde Fowler<sup>2</sup> flatly denied that there ever could have been any connection between the Terminalia and the end of the year, and this on the ground that "Terminus is the god of the boundaries of

<sup>1</sup> For if you are going to make an addition after the last day of the year, manifestly you must first have determined what the last day of the year is.

<sup>2</sup> In his *Roman Festivals of the Period of the Republic*, p. 324.

land, and has nothing to do with time; and the Terminalia is not the last festival of the year in the oldest calendars. The Romans must have been misled by the coincidence of the day of Terminus with the last day before intercalation." Why intercalation was made at this point, Mr. Warde Fowler does not attempt to explain, though one would think that some mention of the question was clamorously demanded in view of his summary rejection of the ancient testimony.

But what has been said and intimated above ought to be sufficient to indicate at least the possibility of error in making such a categorical statement as Mr. Warde Fowler did about the limitation of the functions of Terminus: therefore there is no reason for believing that Varro and Ovid (Verrius Flaccus?) were misled by the position of Terminalia with reference to the intercalations. Why not believe rather that they said what they did simply because it was so, though it had been generally forgotten? An explanation is not properly an object of suspicion because it explains, and does it simply and well.

But Mr. Warde Fowler said that "the Terminalia is not the last festival of the year in the oldest calendars." He of course had reference to the fact that in the calendars February is a month of twenty-eight days, and the festivals of Regifugium and Equirria occur in it later than Terminalia. His statement is therefore perfectly true, but not such an obstacle as the propounder felt it to the acceptance of what he acknowledges was the belief of the Roman scholars. The calendars in the form in which they have been handed down to us all date in certain portions from a period very early, to be sure, but from the time when the calendar had been adjusted with fair precision to the computation of a lunar-solar year. That certainly could not have been its original condition. It is not at all unreasonable to accept the evidence as sufficient that at the earlier epoch the day of the Terminalia, the day later designated as February 23, was indeed the last day of the calendar year. It is of course impossible to say how many days the year had at that exceedingly ancient time, nor how they were divided (if at all) into months, though some not uninteresting guesses about such things are possible. But into that matter I am not disposed to enter at present.

Mr. Warde Fowler's objections appear to me, therefore, to be not well taken. Let us turn back for a moment to those advanced so long ago by Hartmann.

Hartmann said that the statement that the Terminalia had once been the last day of the calendar year was incredible, because a month of twenty-three days could not be reconciled with anything like the true length of a lunation, and, furthermore, none of the ancients spoke of a month of that brevity. On that latter point he of course appears to be in some logical difficulty; for if Varro and Ovid (Verrius Flaccus?) spoke of Terminalia as once the last day of the year, they at any rate (whatever may be the case with others) must be understood thereby implicitly to affirm that the last month of the year had at that primitive time only twenty-three days. It is true that twenty-three days is altogether too short a time to constitute a lunar month; but the sufficient answer to Hartmann's first objection is, as has been indicated just above, that we are dealing with a period lying far back of that marked by the Roman's ability to determine that twelve lunations correspond to a period of 354 or 355 days. We may even conjecture, as will be said later, that it antedated the division of the year into twelve months. The confused statements of the ancient chronologers about the condition of the calendar in the time of "Romulus," which was to them the primitive epoch in their history, are quite consonant with this surmise. The period when Terminalia was regarded as actually the last day of the year must have ended well before a twenty-eight-day February came in, though the earlier condition of things continued permanently to be recognized by the insertion of the intercalary days, when any insertion was made, immediately after Terminalia.

I believe, therefore, that those ancient scholars were right in saying that Terminalia was once the last day of the year. I think I should believe it even if they had not said so. There is no sufficient reason to doubt it. The puzzle of the place of the intercalations is thus solved in the most simple and natural way. The fact that the ancients did not make the assertion about Terminalia in connection with any attempt to account for the place of the intercalations is, if true, an indication in favor of the trustworthiness of their statement; this had no ulterior motive, unless to correct the popular misappre-

hension that the Terminalia had to do from all antiquity with Terminus as a guardian only of land-boundaries.

We may now at last turn to some consideration of the Regifugium.

This festival is assigned in the extant inscribed calendars and elsewhere to the twenty-fourth of February (*a.d. VI Kal. Mart.*), the very next day after the Terminalia. This is in itself a somewhat striking thing, since in no other instance through the whole year do two of the great and most ancient festivals of the whole people—*feriae publicae*, those that have their names inscribed in large letters—fall on consecutive days. Moreover, in only one other instance, and that in some degree explicable, does one of these festivals fall on an even-numbered day of any month (the Equirria of March, on the fourteenth).

Regifugium evidently means the flight, or the putting to flight, of a king, or of *the* king. The ancient Romans of the classical and later period believed the day to commemorate the dethronement of the last King Tarquin. So, for example, Ovid understood the matter,<sup>1</sup> and so did even the Calendar of Siluius; also the Praenestine Calendar,<sup>2</sup> for which Verrius Flaccus is believed to have been responsible, mentions it as the common belief, though only to contradict it. The epitome by Paulus says merely that the Romans called a certain sacred day "Regifugium," because on that day King Tarquin fled from Rome.<sup>3</sup> Festus had a much longer notice, but the passage has been greatly mutilated. Various scholars have attempted to restore it, especially by recurring to the note appended in the Praenestine Calendar to March 24. This is a proper-enough source, for Festus is epitomizing Verrius Flaccus, and Verrius Flaccus is believed to be responsible for the matter of the Praenestine Calendar. The quality of the day (March 24) is indicated in the Calendars by the letters Q.R.C.F, or in two of them (Maffei and Praenestine) by Q.REX.C.F. The note in the Praenestine says that most people have wrongly believed that this day was so marked *quod eo die ex comitio fugerit*

<sup>1</sup> *Ou. Fast.* II. 685.

<sup>2</sup> The ancient calendars are edited by Mommsen in the first volume of the *Corpus Inscriptionum Latinarum* (second edition preferable).

<sup>3</sup> Paul, p. 279 (Mueller), p. 385 (Ponor), p. 347 (Lindsay) *Regifugium sacrum dicebant, quo die rex Tarquinius fugerit a Roma* (Mr. Warde Fowler misunderstood the way in which Paulus used *dicebant*).

[*rex*]. That is, they thought the letters Q.R.C.F. to stand for QVANDO REX [*ex*] COMITIO FVGIT. Verrius (if it was he) goes on very sensibly to remark that Tarquin did not flee from the Roman Comitium [he was besieging Ardea when the Romans deposed him], and furthermore, a day in another month [May 24] is marked with the very same letters. The true meaning in the judgment of Verrius is that after the adjournment of the *comitia* on that day, the courts might be opened for business.<sup>1</sup> This understanding of the mystic initials is made clear by Varro, who says<sup>2</sup> that they stand for QVANDO REX COMITIAVIT FAS, and explains in similar manner to that of Verrius. It is evident that the people had falsely patched up their understanding of the letters from the name Regifugium (it will be noted that REX actually occurs at full length in two of the Calendars). I do not think it could have been from any knowledge of the ceremonies of that day, though that of course is possible.

The passage of Festus on the Regifugium, as best restored, says that the popular notion that Regifugium commemorates the flight of Tarquin from the city on this day is altogether wrong, since history narrates that he went into exile from the camp; a better explanation can be given by him who knows that on this day the King and the Salii perform a sacrifice in the Comitium, upon the completion of which the King flees from the place. Then Festus went on apparently to explain, like Verrius in the Praenestine Calendar, the meaning of the note Q.R.C.F.

From all this it would appear that on the day of the Regifugium a sacrifice of some sort was offered, apparently in the Comitium, the primitive gathering-place of the people. At this sacrifice the King (in later days the *rex sacrorum*) was at least present. The very ancient priestly brotherhood of the Salii assisted at the rites. That would in itself appear to attribute to the origin of the service a

<sup>1</sup> The gathering of the people on this occasion would probably be in *comitia calata*, under the presidency of the *rex* (or later the *rex sacrorum*) for certain expiatory sacrifices. Mommsen appears to be wrong in thinking the meetings of the *comitia calata* for the proving of wills were held on these days.

<sup>2</sup> Varr. *L. L.* VI. 81 *dies qui uocatur sic*, "QVANDO REX COMITIAVIT FAS," *is dictus ab eo, quod eo die rex sacrificulus litat ad comitium, ad quod tempus est nefas, ab eo fas: itaque post id tempus lege actum saepe.*



very high antiquity, far surpassing that assigned to the expulsion of the Tarquins. At the conclusion of the sacrifice the King ran out of the Comitium, as if in flight. That may have been the last act of the ceremonial, or there may have been something more to follow, a knowledge of which would have been of the keenest interest to us. We should certainly like to know what sort of a victim was sacrificed, and if it was really slain, or only subjected to a form of slaying.

We certainly have to do here with a very antique ceremony, which had become a mere piece of inert ritual so long before what we may call historical times that all remembrance of its origin and meaning had utterly vanished. The most learned of the ancients, while they properly deny the reasonableness of connecting it with the expulsion of the Tarquins, yet are able to furnish us with no explanation of its significance, and with only a hint of its form. Probably we should be no better off if we had at command all that they ever wrote on the topic.

Less learned ancients mixed up the Regifugium not only with the two Q.R.C.F-days, but with another very old festival even less known to us, that was called "Poplifugia" (July 5). We may guess that the similarity between the endings *-fugium* and *-fugia* (why were they not precisely alike?) was responsible for this, rather than any resemblance known in antiquity between the ceremonies of the two days. Of any ceremonies at all on the Poplifugia we know nothing, though the ancients proffered in pseudo-explanation so many guesses about times in their past history when the people fled that one might have supposed it likely that they would drop some word about the rites of the day, if they had themselves known that any existed. It should be noted that neither in the case of the Regifugium did the people who spoke of the flight of King Tarquin appear to know of any existing ceremonies on that day; only the great scholars did that, and even they leave us entirely in the lurch about the Poplifugia.

The King ran away at the end of the rites of the Regifugium. But why should he run? Some modern scholars have essayed to answer that question by reference to certain ancient rituals that involved condemnation or outlawry for killing a sacred victim, even in due form of sacrifice. They have also invoked the concept of a

lustration and a scapegoat. None of these explanations appear to me satisfactory, though I must not here take space to discuss them.

But Sir James G. Frazer has offered in successive editions of his now famous *Golden Bough*, as well as in his *Lectures on the Early History of the Kingship*,<sup>1</sup> a most illuminating and convincing conjecture regarding the origin and nature of the name and rites of the Regifugium. He suggests that we have in them a survival, even though a faint and shadowy survival, of a custom that he shows by sufficient examples existed, or in some form still exists, among many primitive tribes, and even among Indo-European peoples. In some such communities, which were still in that primitive stage of culture when magic had not yet passed into, or been displaced by, religion, there was a man who was regarded as the incarnation of the mysterious spirit of all vegetation. On his strength and vitality depended the success of the farmer's labors. He was therefore regarded as divine. He bore the title of "King," and was free from all control and restraint during his reign. But that reign was brief. It lasted but a single year, ending shortly before the approach of seedtime. He was then put to death as a sacrificial victim, or forced to immolate himself, and a successor took his place. For it was deemed necessary that the King's physical powers should never be permitted to pass into decline. That would bring ruin upon the crops, and perhaps also sterility upon flocks and herds, or even upon the human race. At what we may presume to be a later time, instead of being sacrificed, the King was permitted to prove, if he could, the retention of his pre-eminent strength by a duel to the death with an opponent, who, if successful, became King in his stead for the following year. The flight of the King perhaps marks a later stage yet in the history of the ordeal, and may indicate that if he could demonstrate his continued vigor by successful escape from his would-be executioner and successor, he might be permitted to prolong his reign for another year without further molestation. So by gradual stages the annual test becomes a mere form, and the King reigns for the term of his life.

I am disposed to accept most heartily and gratefully the brilliant suggestion of Dr. Frazer concerning the ultimate origin of the Roman

<sup>1</sup> See the indexes to the respective works *s.u.* Regifugium.

Regifugium, and am here concerned only to make certain remarks about its position in the Roman calendar, on which Dr. Frazer says nothing.

The year naturally opened with the approach of springtime and the renewal of active farm operations. The end of the annual King's reign would accordingly come just before that period—in terms of the later calendar, shortly before the Kalends of March. These Kalends marked the inauguration of the new King. With this agree the facts that in historical times the *rex sacrorum* appears to have entered upon his lifetime office on the first of March,<sup>1</sup> and on that day in each year the sacred fire in Vesta's temple was formally rekindled.<sup>2</sup> On that day also the Regia, the houses of Vesta and of the flamens, and the Curiae Veteres were decorated with fresh laurels.<sup>3</sup> The Vestals were originally closely connected with the King, as the daughters of the household with the father; and one may well imagine that the renewal of Vesta's fire on the first of March denoted the starting-point of a new reign under new auspices. Each year is in itself a completed epoch: the old has passed away, the new is begun.

The Terminalia was indeed the end of the year; but the death of the King was not carried out on that day, since that would cut short the full year's reign to which he was entitled. Therefore the following day was that decreed for his fate. This twenty-fourth of February, later known as the Regifugium, we may presume was accordingly in primitive times not a part of the year. It stood unattached, after the end of the old year, and before the beginning of the new. It was followed, as early as we can trace it, by four days presumably similarly situated, in which the new King might consult the gods, and get their formal approval before his inauguration on the first of March. This was apparently the origin of the *interregnum*, that period of five days which the Roman antiquaries

<sup>1</sup> Cf. Hushke, *op. cit.*, pp. 37 ff.; Hartmann, *op. cit.*, pp. 229 f.

<sup>2</sup> Cf. *Ou. Fast.* III. 143 f.; *Macrob. Sat.* I. 12. 6; *Sol.* 1. 35. It appears possible that in the very primitive times with which we are here dealing, Vesta's fire was put out, or allowed to go out, at the end of the Terminalia, to be rekindled again only after the *interregnum*. The ancients tell us merely that the fire was renewed on the Kalends of March. They do not tell us when it went out.

<sup>3</sup> Cf. *Ou. Fast.* III. 135 ff.; *Macrob., Sol., loc. cit.*

said was the minimum time that must always elapse between the death of the old King and the installation of his successor, in that regal period which they thought historic.

But if there was an intercalation at the end of the year, it of course came immediately after Terminalia, and formed a part of the year. The King would therefore be allowed those extra days of life, and the Regifugium would follow immediately thereafter, and be itself followed by the other four days of the *interregnum*.

The two anomalies already mentioned of having the Regifugium placed in immediate succession to another of the most ancient class of festivals, and on an even-numbered day of the month, are thus satisfactorily accounted for.

A February of twenty-eight days violated the ancient rule, on which the Roman writers about the primitive calendar laid great stress, that each month should consist of an odd number of days. We may therefore reasonably conjecture that the five days of the primitive *interregnum* (February 24-28) were not formally incorporated into the month of February until all lively sense of the origin and significance of Terminalia as the last day of the year, and of Regifugium as the day of the King's death, and of the four days that followed as a part of an extra-calendar *interregnum* had died away, and also the superstition about odd-numbered days was beginning to break down, or had altogether broken down. It was in full vigor when Terminalia was recognized as the last day of the year; it had of course utterly vanished for all practical purposes when Caesar reformed the calendar, and assigned thirty days to certain of the months. Perhaps it was thought by Caesar (who was himself Pontifex Maximus) that since February had for centuries had twenty-eight days, there could no longer exist any valid religious scruple against giving other months an even number of days.

This incorporation of the five interregnal days into February may perhaps be dated from the time when the days of the year first began to be divided systematically into months that conformed in a more precise manner than was earlier possible to the pattern of a lunar year.

When intercalations were necessary, they of course continued to be made in what was formerly their proper place, immediately after

Terminalia, the last day of the primitive year. There could of course be no thought in the Roman mind of a transfer of the festival of the Terminalia from the twenty-third to the twenty-eighth of February. That would have been no more possible than to transfer Terminus himself.

The more ancient method of dating the days between the Ides of February and the end of the year (Terminalia) from Terminalia instead of from the Kalends of March, of which the few traces already mentioned still existed in much later centuries, probably continued to be more or less in popular use, though without any surviving notion of its ultimate origin.

Macrobius said correctly enough that the last five days of February (24-28) were added after the intercalation, but he assigned as the possible reason that there was ancient religious scruple against having March preceded immediately by anything else than February.<sup>1</sup> That appears to be, as indeed his *credo* indicates, a mere guess on his part, and a wrong one. But of course he had no inkling of what we may now reasonably conjecture to be the true history of the Roman calendar with regard to the Terminalia and the succeeding days of February.

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<sup>1</sup> Macrobius, *Sat.* I. 13. 15 *deinde reliquos Februarii mensis dies, qui erant quinque, post intercalationem subiungebant, credo ueteris religionis suae more, ut Februarium omni modo Martius consequeretur.*

## MEGES AND DULICHIMUM

By A. SHEWAN

Three leading chapters of Dr. Leaf's *Homer and History* contain the most determined attack on the Catalogue, or *Boeotia*, in the *Iliad* that has ever been made. Its author examines the accounts given by the "Cataloguer"—as he styles the villain of that piece—of the Dominions of Agamemnon, Peleus, and Odysseus, and proves to his own satisfaction that these were written at a late period, and present pictures of political conditions which never existed, but which were conjured up by the Cataloguer for purposes of his own. I have given reasons elsewhere for discrediting this view as regards the realms of Agamemnon and Peleus. For that of Odysseus, Dr. Leaf accepts with his whole heart Dörpfeld's theory that by "Ithaka" Homer meant Leukas or S. Maura. That is a question that has remained in abeyance, pending the appearance of the volume on the subject that Dörpfeld promised nearly twenty years ago, and that is still, it is understood, in preparation, and any full discussion of it as a whole may well await its author's full and final statement. Dr. Leaf's own account of it is mostly on familiar lines, but he has also some new and original evidence of his own. This is the case with the kingdom, and the position in the world of Homer, of Meges, whom the Cataloguer makes ruler of Dulichium. In no part of his exposition of the enormities wrought by that reprobate is Dr. Leaf more positive. His tone is in fact so confident as to suggest that it is futile to entertain any other view than the one he propounds. It is "as clear as the day that the poet of the Odyssey has never heard of Meges at Dulichium" (*Homer and History*, p. 159), and the facts are "in flat contradiction to the theory which would find Dulichion in Leukas" (p. 153), and so on. I propose to consider the evidence on which he relies for these irrefragable conclusions.

Dr. Leaf's procedure is to show differences between the Catalogue in the *Iliad* and "Homer," and he finds such (p. 158) in regard to Meges. The Catalogue shows that "Odysseus," in the opening of  $\iota$ , "was in error in treating the Four Isles," Dulichium, Samé, Zakyn-



thos, and Ithaka, "as a homogeneous group," and, worse than that, "he grossly misleads us when he gives us to suppose that he was the chief man, or even a very important one, in the Four Isles," for he brings to Troy only twelve ships against the forty of Meges. The last part of the statement will be referred to farther on, but the rest of it cannot be considered correct in any particular. "Homogeneous" is a good word, but it is not applicable here. The homogeneity that can be predicated on the references in the *Odyssey* is no more than this, that the four islands are mentioned together four times, evidently as the principal islands off the west coast of Greece—three times as supplying Wooers, and once in the definition of the position of Ithaka. If what Dr. Leaf wishes to believe, and wishes us to believe, is that the *Odyssey* makes the group a homogeneous Dominion under Odysseus, it is a common but mistaken view for which the *Odyssey* gives no warrant.<sup>1</sup> Dr. Leaf admits (p. 140) that there is no express statement in the poem that Odysseus is king of the four islands; he infers it from the speeches of Telemachus and Mentor in β 70 ff. and 233 ff. But it is difficult to find in these passages any ground for such an inference. And there is, I think, a very serious mistake. Dr. Leaf's translation of λαῶν, β 234, by "peoples" will never do. Homer, or I think any other Greek author, could not use λαοί to mean a number of communities, that is, in the sense in which θῆρα or φύλα or the like is used. And Dr. Leaf has to assume that "all the suitors, whencesoever they came, are on the same footing as chieftains under the common rule of Odysseus," which seems to be *petitio principii*. Finally, it may be asked where, in his account of himself in the opening of ι does Odysseus state that he is the chief man in the four islands. He seems only to inform his hosts that his native island is Ithaka, and that he is well known to the world for his wiles.

Another argument is (p. 158) that Meges is not mentioned at all in the *Odyssey*, whether as king of Dulichium or otherwise. But that is a point worth making only if it can be shown that there is some reason, which the author of the poem has disregarded, why Meges *should* be mentioned. It would make the argument stronger if it could be shown that in the *Odyssey* someone else has taken his

<sup>1</sup> See, e.g., Mr. Allen in *JHS*, XXX, 105.

place, and that Meges "is excluded," but that, as we shall see farther on, Dr. Leaf fails to prove. It may be said that Nestor might have named Meges in γ 184 ff. There the old man begins by saying that of many of the chiefs he has not heard, οἳ τ' ἐσάωθεν Ἀχαιῶν οἳ τ' ἀπόλοντο, and adds that "they say" the Myrmidons, Philoctetes, Idomeneus, and Agamemnon have returned safe. These are outstanding names, and Nestor obviously omits reference to many besides Meges. It will of course be replied that Meges was a neighbor, and that Nestor ought to have known about him. Perhaps he ought, but other leaders, as Agapenor, Thaplios, and Polyxenos, are not named, and *their* territories were much nearer Nestor than Dulichium. The point is hardly worth discussing. Meges is large in Dr. Leaf's view because of the importance of Dulichium in the Leukas-Ithaka controversy, but his position and deeds in the *Troica* are not such as to make it imperative that he should be distinguished by mention in the *Odyssey*. Mr. Allen (*The Homeric Catalogue*, 83) speculates on his fate, but it does not seem to matter whether he was dead, back in his home but married—in which case he would not be wooing; one account gave him Odysseus' sister to wife—or still dreeing a πολυκηδέα νόστον. Homer did not need or choose to bring him into the story of the *Odyssey*, and that is all that needs to be said. In Homeric discussions inferences are not so readily drawn now as formerly from Homer's omission to say something necessary for a critic's theory. The opportunities for naming the ruler of Δουλίχιον in the very few passages in which the name occurs may be judged by reference to those passages. It may be said there is just one worth considering, in the tale told to Eumaeus, and that will be noticed presently.

In regard to the realm of Meges, Dr. Leaf's first argument (p. 166) is that in the Catalogue, B 625, Δουλιχίοιο Ἐχινάων θ' means the island of Doliché (in the Echinades) and the other Echinades, because in another section of the Catalogue Βουπράσιόν τε καὶ Ἥλιδα means "quite unmistakably Buprasium and the rest of Elis." One similar instance is surely little enough on which to decide, even if it were certain that Δολίχη and Δουλίχιον are one and the same, and that the Cataloguer means by "Elis" the country known by that name in later times.

His description is peculiar; indeed the language is not easy to construe.<sup>1</sup> He is describing a tract in the northwest of the Peloponnesus inhabited by Epeioi, but that the whole of that tract was "Elis" to him cannot by any means, so far as I can discover, be regarded as established. And this much may be added, if the Cataloguer sent Meges to Troy with the respectable contingent of forty ships from the Echinades alone—the Echinades, which Dr. Leaf labors hard to prove, by copious quotation from the *Mediterranean Pilot*, a "poor cluster of rocks," then we must surely write him down a fool as well as unscrupulous rogue. Dr. Leaf's interpretation of Δουλιχίου Ἐχινάων θ' cannot be accepted. We must inquire further.

Dr. Leaf despises the Echinades. Now it may be true of them at the present day that their hills are bare, and as useless, if you please, to man and beast as "the barren rocks of Aden," but it does not follow that they have always been so. That is a fallacy that appears more than once in *Homer and History*.<sup>2</sup> Nothing is more certain than that, in that southern climate with a good rainfall, every one of those islands was once covered with forest and blessed with abundant springs. Zakynthos was, according to Homer, ὑλήεσσα, and Ithaka had ὕλη παντοίη and ἀρδμοὶ ἐπηετανοί, ν 247, though man has destroyed the wood and thus largely deprived himself of the water. The Echinades were in Homeric days of some use to the population. Pasture and wood would certainly be plentiful. Also, all down the coast from Krithoté at the northern end of the string of islets there are *treffliche Häfen*.<sup>3</sup> And one fact of prime importance is not to be overlooked—the hinterland, that is, the coast of Acarnania, and the rest of that country are not included in the Catalogue. Aetolia comes up to the Achelous. Between that river and the coast were, as Dr. Leaf describes them (pp. 167 f.), savage tribes, who took no part in the *Troica*. They were not of Greek nationality; their Hellenization took place only after Corinth established colonies in Western Greece and the islands.<sup>4</sup> Very possibly they were, as later,

<sup>1</sup> See, e.g., Ameis-Hentze, *Anhang*, and Allen's summary, *op. cit.*, pp. 80 f.

<sup>2</sup> See *CR*, XXX, 81, and XXXII, 4, and *CP*, XII, 134 f.

<sup>3</sup> Bursian, *Geogr. von Griechenland*, I, 118.

<sup>4</sup> Kretschmer, *Einleitung*, pp. 254, 257.

continually fighting the Aetolians, the *συνέχεια* of whose wars is referred to by Eustathius, 1321, on Ψ 630. As Bursian says,<sup>1</sup> these quarrels were the *Mittelpunkt* of the history of Acarnania, and that they began early we know from the story of Meleager and the Kuretes in the 9th *Iliad*. Acarnania was in an unsettled condition, with its population occupied with enemies on its eastern frontier, and it is not surprising to read in the Catalogue and the *Odyssey* alike that Odysseus had a footing on the coast. That he kept an establishment there we know from ξ 100-2. On *ἐπινομία* see Seymour, *Life in the Heroic Age*, p. 240.<sup>2</sup> If then the Dulichium of the Catalogue be S. Maura, the likelihood cannot be denied that Meges, lord of the Echinades and Dulichium, had a footing too, especially as S. Maura is really part of Acarnania. It is true, as Eustathius remarks, 1225, on Φ 86, that the poet in the Catalogue *ἐσίγησε τοὺς ὑπὸ τῷ Ὀδυσσεὶ Ἀκαρνᾶνας*, just as he omits other particulars which are to appear elsewhere in the *Iliad*. There are numerous instances of the kind, and there is no reason to be surprised at the procedure. I think the explanation of Eustathius, 622, on Ζ 23, is quite satisfying. The poet might have crammed all details into the Catalogue—*μὴ ποιήσας δὲ τοῦτο, ἀλλὰ ταμεινσάμενος αὐτὰς (ἱστορίας) ἑκασταχοῦ, ἔνθα καιρὸς, εἰς ποικιλίαν τε τῆς ποιησέως, καὶ ἵνα μὴ ὁ κατάλογος ἐκείνος μακρότερος τοῦ δέοντος γένοιτο*. And surely, if the Cataloguer was a late and rash intruder, one must wonder why he did not give more verisimilitude to the contents of the document he was concocting, by making it harmonize better with the two poems for which he was preparing his list. Fick,<sup>3</sup> I may add, says that by Dulichium in the Catalogue *zweifelloos Akarnanien gemeint ist*, which seems to go too far. But if Dulichium is S. Maura, the arrangement of territory suggested above is not only an intelligible one, but also not inconsistent with either the Catalogue or the *Odyssey*.

To Dr. Leaf, Dulichium is Cefalonia, for (p. 347) Homer brings from Dulichium "the largest number of suitors, the Cataloguer the largest number of ships," so that "both regard Dulichion as the largest island of the group." No such conclusion can be drawn with

<sup>1</sup> *Op. cit.*, I, 108.

<sup>2</sup> Where he says, "clearly Odysseus had rights of pasturage on the mainland."

<sup>3</sup> *Ortenamen*, III, and cf. 130 and 139.

any certainty, for a smaller island may have a larger cultivable area and be more cultivated, may have a better and more extensive trade, may consequently sustain a larger population, and may be altogether more advanced than a larger one. Nor is the comparison of the populations of modern Cefalonia and S. Maura, 70,000 and 30,000 respectively, of any use for the ancient times with which we are concerned. As to the number of ships, Mr. Allen's observation in *JHS*, XXX, 304, is worth recalling. Perhaps the Cephallenēs "did not respond largely to Agamemnon's call. We remember Ulysses' own reluctance." The reluctance may be reflected in the small contingent of ships supplied by the three islands, Samos, Ithaka, and Zakynthos. Finally, though there were Δουλιχῶται, and there is a Doulico, in Cefalonia, per contra it had Samioi and a town Samos or Samé.<sup>1</sup> But a discussion of the Cefalonia-Dulichium equation would take me beyond my limits, and it has been well threshed out already in this controversy. When I come to consider the words πρὸς Ἡλῆδος in  $\phi$  347, we shall find reason for holding that the Samé of the *Odyssey* is Cefalonia, as it was always taken to be *ante Dorpfeldiam litem motam*.

Dr. Leaf considers (p. 165) the point, "why this poor cluster of rocks, the Echinae, have been found worthy of mention in the Catalogue at all," and thinks a probable answer is this, that the district in which the islands lie had a legendary history all to itself—the expedition of Amphytryon of Thebes against the Teleboai and the purification of Alkmaion—which connects it directly with Boeotia. "It is therefore natural that to the Boeotian Cataloguer" the Echinae should possess a traditional importance quite out of proportion to their real quality. This is only "a suggestion" (p. 319) of a possibility. The notion of a Boeotian origin for the Catalogue dies hard. Mr. Allen's refutation<sup>2</sup> should, I think, be final. One of Dr. Leaf's arguments (with others of a minor kind) for the Boeotian connection, based on the gathering at Aulis, was examined in detail in *CR*, XXXI, 37 ff., and found to be of no value.

But Dr. Leaf's Dulichium case is far from being exhausted. He goes further, but seems to me to fare no better. I pass to his reference to Amphinomos. I am not sure whether on page 158 he means

<sup>1</sup> Mr. Allen, *op. cit.*, pp. 85, 89, and notes.

<sup>2</sup> *Op. cit.*, pp. 41 ff.

by the translation of Νίσου ἀνακτος, "Nisos the prince" (father of Amphinomos), and by his remark that there is in the passage "no allusion to Meges," that Amphinomos, or Nisos if alive, had supplanted the Cataloguer's Meges. Whether or no, it is clear there is no ground for arguing that either father or son was a ruling monarch. Amphinomos was only the principal one among the Wooers from Dulichium, ὅς ῥ' ἐκ Δουλιχίου . . . ἡγείτο μνηστῆρσι, π 396 f. In that there is no contradiction of the Catalogue. If Meges was alive and had accomplished his νόστος, he may not have cared to woo, or having a wife alive, may have been prevented. Amphinomos, it may be admitted, was, like Antinoos and Eurymachos of Ithaka, of the rank of βασιλεύς, but neither of these was king, and we may infer the same for Amphinomos. He and his father have the description Δουλιχεύς. That is how the disguised Odysseus refers to Nisos, σ 127. He has heard Νίσον Δουλιχιῆα ἐν τ' ἔμει ἀφνειὸν τε. That does not take us far on the road to sovereignty. It might almost be argued from it alone that he was something less than monarch. I think some might even draw that inference from the words of Amphinomos himself in π 401 f., δεινὸν δὲ γένος βασιλῆϊόν ἐστι κτείνειν. It does sound as if the reigning family in Ithaka were somewhat βασιλεύτεροι than the speaker.

But, whatever Dr. Leaf means with regard to Amphinomos, he is clear on the same page that there is a king of Dulichium in the *Odyssey* who is not the Meges of the Cataloguer. This is one Akastos, mentioned ξ 336. The name occurs in the story which Odysseus in beggar's guise told Eumaeus. Pheidon, Θεσπρωτῶν βασιλεύς sent him on by ship ἐς Δουλίχιον . . . βασιλῆϊ Ἀκάστῳ. On this Dr. Leaf, Mr. Allen, and others accept Akastos as king of Dulichium, but there is no warrant for such a conclusion. There were πολλοὶ βασιλῆες in Homer's Ithaka, α 394 f., and a dozen at least, θ 390, in his Scheria farther up the coast. Why not in Dulichium? In Ithaka Antinoos and Eurymachos were βασιλῆες, σ 64 f. Pheidon of Thesprotia is ἀναξ, ξ 326, but even that title does not prove royalty. He has, it is true, the description Θεσπρωτῶν βασιλεύς, but it is most unlikely that the "numerous tribes and clans" there<sup>1</sup> were under one supreme authority. But, be that as it may, there is certainly no reason for

<sup>1</sup> Kretschmer, *op. cit.*, p. 245.



holding that Akastos was more than a βασιλεύς. If he is, on this description, king of Dulichium, then Echetos (σ 85, 116, φ 308) is king of the ἡπειρος, or Acarnania, for he is in all three passages Ἐχέτος βασιλεύς, just what Akastos is. To Dr. Leaf, Echetos is a "bogey king"; on the contrary, he seems to be a very real personage, and there is no reason whatever for holding him to be a bogey. It is, I repeat, unlikely there was a king of the "rough hill-tribes"—Dr. Leaf's own description—in either Epirus or Acarnania. And surely the caution of Seymour<sup>1</sup> should be borne in mind, as particularly applicable to the countries now under reference, "the story of the poems does not require exact statements as to governments." In some cases exact statements could not be made. Least of all should we look for such in the idle tales of the *soi-disant* wandering beggar.

But as Dr. Leaf has dragged Akastos into this crusade against the Catalogue, I am inclined to ask whether there ever was such a person. I think he was unreal. Let us look into the accounts which Odysseus gives of himself while incognito. I take first his story to his own father in ω 303 ff. It is that he is son of a wealthy man, Apheidas Polypaemonides, that his own name is Epēritos, and that he comes from Alybas. The ancients localized Alybas in various directions. The moderns—see, e.g., the notes of Monro, van Leeuwen, Merry, and Ameis-Hentze—believe that its name is a fiction (ἀλάομαι, ἀλώω, or Argyropolis, van L.), and the personal names the same. And there is this to be noted, that all these names can be taken as having a reference to Epirus. For the father's name, Apheidas, I find there was a tribe of Apheidantes in that country.<sup>2</sup> For Epēritos the similarity to Epirus is plain enough. For Alybas I find that the very similar Arybbas was an "Epirotic personal name."<sup>3</sup> For the termination cf. Karnabas son of Triopas, Περραιβῶν τύραννος—there were Perrhaebi in Thesprotia—(Eustathius, 448 on Δ 87 ff.). And there is some reason for the statement of Fick (*Personenn.*, p. 431) that *Odysseus und sein Geschlecht ursprünglich den Thesproten angehörten*. It seems to me that in this tale Odysseus is, in imagination, in Thesprotia, and every detail is invented. Is it not the same with

<sup>1</sup> *Op. cit.*, p. 96.

<sup>2</sup> Bursian, *op. cit.*, I, 24, and Fick, *Personennamen*, p. 412.

<sup>3</sup> Kretschmer, *op. cit.*, p. 258, n.

the tale to Eumaeus in ξ? It is on the same lines as the other. Again he is the son of a wealthy man, Kastor Hylakides, ξ 200, 206, and a Cretan, as he is to Athené in ν 256 ff., and this time he, or Homer for him, connects his life-story with Thesprotia directly. If he was inventing names or using known ones at random in ω, it may well be that he is doing the same in ξ. A slight indication that in the latter Homer is using any names that come to hand is to be found in the likeness between Kastor and Akastos, not much in itself, it is true, but a little more striking when we note the same amount of likeness between the Pheidon of one tale and the Apheidas of the other. So that it seems likely enough that Akastos is a mere fiction. Eumaeus need not be assumed to have known all the βασιλῆες up and down the coast. The fictions of Odysseus were ψεύδεα ἐτύμοισιν ὁμοῖα, and some of the personal names now under reference were names in actual use. I think further study of them might be productive of some result, but all I suggest here is that there is good ground for the belief that the πολύτροπος one is having some fun to himself with Eumaeus, as he is with his father in ω (κερτομίους ἐπέεσσι, 240), and as he is with the Cyclops in ι. There is certainly enough to warn us against taking Akastos seriously. For that reason, and especially because βασιλεύς does not necessarily imply sovereignty, we appear to be justified in removing him from the scene as Dr. Leaf's "king of" Dulichium, and allowing Meges to continue to reign there in peace.

The flim-flam told to Eumaeus in ξ 314-59 needs further notice. Dr. Leaf (p. 349), like Mr. Brewster<sup>1</sup> and others, has yet another argument based on it. The beggar guest tells Eumaeus that Pheidon of Thesprotia put him on board a Thesprotian ship, which was to take him to Dulichium. If then, it is said, S. Maura is Dulichium, all the shipmen had to do was to run him down the coast to its northern shore. Instead, we find them at Ithaka. If Thiaki, south of S. Maura, be Homer's Ithaka, what were they doing so far out of their way? Dulichium must be south of S. Maura.

The argument ignores the possibility that they were making for Vlichos, the port on the east coast of S. Maura, and were following what may have been an ordinary course for that end. But let that

<sup>1</sup> *Harvard Studies*, XXXIII, 65 f.

pass. It is certain they did take the long course, otherwise there is no meaning to be got from ξ 339, ἀλλ' ὅτε γαίης πολλὸν ἀπέπλω ποντοπόρος νηὺς, a line which appears to be overlooked. The necessity for going "far from land" can be explained only in this way. But there is a worse omission in the argument. No account is taken of the κακὴ βουλή formed by the ship's company. As soon as the ship was well away from the land, they decided to sell their passenger as a slave, and to begin with stripped him of his fine raiment. At evening they arrived at Ithaka. Dr. Leaf (p. 349) and others say they touched there *in order to get supper*, and it is regarded as an absurdity that the sailors should have gone so far out of their way simply in order to have supper on that island. So it is. But would not the κακὴ βουλή explain all to Eumaeus, even if he were, like a modern critic, scrutinizing every detail of a story which, as soon as it began to tell of his old master, be regarded as the purest fiction (ξ 363 ff.)? Would it be safe for the sailors to sell their slave in Dulichium, when he had been sent in their care to a βασιλεύς there? That must be done at a distance. They are Thesprotians themselves and must return to their country, and they must also do their business in Dulichium. They can easily have a lie ready for both places, but they must dispose of their man first. Where they were going to take him we are not told, just as we certainly are *not* told that they went out of their way in order to enjoy a supper on Ithaka. They did, it is true, go ashore there to sup, leaving their victim on board in bonds. What the next step was to be we do not know, for their prisoner escaped and foiled their evil intention. This κακὴ βουλή is an element in the tale which cannot be neglected. In fact, it makes the narrative consistent with the theory that S. Maura is the Dulichium of Homer.

Next, Dr. Leaf (p. 161) depends on N 685 ff., which he calls a Catalogue, and dignifies by the name of the *Ionia*, because the *Ἰάονες* are mentioned in it. Now that passage does not pretend to be a formal list. It simply mentions the warriors of the various tribes who resisted a particular attack by Hector, but to call it a "Catalogue" will tell with some. Dr. Leaf finds that "the *Boeotia*, the *Ionia*, and the *Iliad* all contradict one another in the most complicated manner," and "the general upshot seems to be this, that the *Iliad*, the *Odyssey*, and the *Ionia* say that Meges was at home in

Elis, the *Boeotia* says he was in Dulichion. Can our conclusion be in doubt?" I take the discrepancies in detail, and find there is no contradiction. All that can be said is that these "Catalogues" are not identical in content. Who but the seeker after discrepancies would expect them to be? But it is a far cry from that to contradiction.

First, in the *Boeotia* the leaders of the Epeioi are four—of three families—Amphimachos and Thaplios, grandsons of Aktor, Diore son of Amarynkeus, and Polyxenos son of Agasthenes and grandson of Augeas. The *Ionía* mentions Meges, Amphion, and Drakios, so "there is not one name in common, and Meges in the *Ionía* is not in Dulichion at all, but is one of the leaders of the people from whom the *Boeotia* makes him a fugitive." The latter remark is hopelessly incorrect, as it was *not* Meges that was a fugitive, but his father Phyleus, and it was *not* the people that Phyleus was a fugitive from, but his father Augeas. There is no reason to suppose that Phyleus, much less Meges, was an outcast to the Epeioi of Elis after the death of Augeas. As to the other objections, if the *Boeotia* were giving an exhaustive list of the leaders of the Epeioi before Troy, there might be some force in the difference between it and the *Ionía*, but obviously it does not pretend to do anything of the sort. It is a rapid description of a piece of country in the northwest of the Peloponnesus, and it is only incidentally mentioned that the people were Epeioi, πολέες δ' ἐμβαινον Ἑπειοί, B 619. There were Epeioi elsewhere, as in Dulichium, and there may have been other chiefs of lower rank than the four principal leaders. We need not be surprised at hearing of Amphion and Drakios in the *Ionía*, for we are not when we are told in II 168 ff.—shall we regard the passage as a "Catalogue" and dub it the *Myrmidonia*?—that there were subordinates of Achilles who are not mentioned in the *Boeotia*, and who have only this one mention in the *Iliad*. As to Meges in the *Ionía* not being "in Dulichium at all," why should anything be said there about his home and realm? Why should he not, being himself of that race, be acting with other Epeioi? So for Otos, an ἀρχὸς Ἑπειῶν, O 519, who is described as comrade of Meges and as belonging to Kyllené in Elis. Might an Epeian from Dulichium not be a friend of an Epeian from Elis? Might they not be comrades at Troy even if they had not known each

other before? But, the argument continues, Kyllené is not mentioned in the *Boeotia*. As to that, Hyrminé, which was close to Kyllené, is mentioned. As an ἀκρωτήριον<sup>1</sup> it may have been a better landmark than Kyllené, and it is, be it observed, points on a boundary—"landmarks," Leaf on B 615—that are given in the *Boeotia*. If the author of the latter was bound to give an exhaustive list of places, we must allow Dr. Leaf all that can be argued from the absence of Kyllené from it. If he was not, the inference is vain.

Yet again Dr. Leaf argues (p. 162) "that to Homer Phyleus himself, the father, was not an exile; for it is in his own land of Elis, that Neleus [Nestor?] met him and outcast him in the spear-throw, when the Pylians faced the Aetolians and Epeians in contest at the funeral games of Amarynkeus (Ψ 630 ff.)," which means that Phyleus could not possibly come from Dulichium for games in Elis. Yet these were attended by Pylians and Aetolians and Iphiklos from Thessaly! Surely material for objection is scarce when such an argument can be used. The criticism by which these "contradictions" are established is really an excellent specimen of the niggling fault-finding, by exaggeration of discrepancies, by which the Germans broke up the poems in the nineteenth century. We understood recently that Dr. Leaf, "rubbing his eyes" over Wilamowitz' latest Homeric book, had broken away from such methods, but one is compelled to ask how his present procedure differs from that of the Berlin scholar. It is surely very unfair in its denial of all freedom to an epic poet.

The expression πρὸς Ἡλίδος in φ 347 has been much discussed, and is one of the Leukadists' trump cards. Dr. Leaf (p. 153) easily wrests it to his own purpose. Telemachus, "when he has occasion to speak of Ithaka in contrast to the rest of the group, denotes the latter as the islands which lie 'on the side of Elis,'" and that is "a perfectly natural expression if Leukas is Ithaka," and not to be reconciled with the Dulichium-Leukas equation. All of which depends on the assumption that Telemachus is referring to the four big islands. Now if we refer to the other three passages in which the four are enumerated, twice by Telemachus, α 245 ff. and π 122 ff., and once by his mother, τ 130 ff., we find the description is a formula,

<sup>1</sup> Bursian, II, 108, and references.

and it is strange that Telemachus does not use it again in  $\phi$ , if he means all four islands. Also there is this difference that, whereas in the three passages Ithaka is named after the other islands, in  $\phi$  it is named first. There is surely some reason for departure from the formula. Indeed, instead of assuming that the four islands are referred to in  $\phi$ , one begins to feel there is ground for presuming they are not.

The better way is to examine the text. Telemachus' words are in the opening of a speech to his mother, when the Wooers are clamoring to prevent the Bow from being handed to his father, and he evidently shares the general excitement. He speaks *to* his mother, but *at* the Wooers, at their leaders indeed, when he says no one has more power over the Bow than he,

*οὐθ' ὅσσοι κραναὴν Ἰθάκην κάτα κοιρανέουσιν,  
οὐθ' ὅσσοι νῆσοισι πρὸς Ἠλίδος ἱπποβότοιο.*

Now who are the leaders referred to in the first line? Antinoos and Eurymachos, as the two principals among the whole horde, especially in this matter of the Bow, could not of course be overlooked, and the general belief is that they were Ithakans. The story about Antinoos' father in  $\pi$  418 ff. seems to indicate that for Antinoos, and the reference in  $\pi$  442 ff. to Odysseus' kindness toward Eurymachos in his childhood seems to show the same for him. See also  $\sigma$  518 ff. and various expressions in  $\chi$  45 ff. These are the leaders at whom Telemachus is speaking in the first line. But who are the leaders from other *νῆσοι* referred to in the second? Not Amphinomos, leader of the troupe from Dulichium; his character excludes him from the outburst of Telemachus. Refer to his action in  $\pi$  394 ff., comparing  $\nu$  245 f., and to the scene in  $\sigma$  119–57, and noting Amphinomos' kindly address in 122 f., and see in addition 394 f., and 412 ff., and note the special testimonial he has from the poet, *φρεσὶ γὰρ κέχρητ' ἀγαθῇσι*. He is a man with real pretensions to uprightness among the ruffianly mob.<sup>1</sup> The second line does not refer to Amphinomos' home, Dulichium; it must refer to the only other islands that supplied Wooers, Samé and Zakynthos, and the only two of the four

<sup>1</sup> The *Mnesteroiphonia* will no doubt be quoted against me. Amphinomos,  $\chi$  89 ff., rushes at Odysseus with drawn sword. The wording is quite consistent with my explanation, for the intent is *not* to kill Odysseus, but only *εἰ πῶς οἱ εἴξει θυράων*.



that lie πρὸς Ἡλιδος are Cefalonia and Zanté. Zanté is Zakynthos, so Samé is Cefalonia. There is thus not only no "flat contradiction" of the theory that Dulichium is S. Maura, but even good support for it. If the equation is correct, we can see another reason for the exclusion of Dulichium from Telemachus' survey. No one would ever think of describing S. Maura by reference to Elis, 55 miles away, and with Thiaki, Arkoudi, and Atoko between.

Another reason for arguing that S. Maura is the Homeric Ithaka is based on the question to a newcomer, as in α 170 ff. (cf. ξ 187 ff., π 57 ff. and 222 ff.):

τίς πόθεν εἰς ἀνδρῶν; πόθι τοι πόλις ἡδὲ τοκῆς;  
ὀπποῖης τ' ἐπὶ νηὸς ἀφίκεο· πῶς δέ σε ναῦται  
ἤγαγον εἰς Ἰθάκην; τίνες ἔμμεναι εὐχετόωντο;  
οὐ μὲν γάρ τί σε πεζὸν ὁλομαι ἐνθάδ' ἰκέσθαι.

This is a point on which Dörpfeld has been answered over and over again. The universal interpretation, till he broached his theory, was that the last line embodied a little joke, "a primitive and rather silly joke" as Dr. Leaf calls it. See, e.g., Eustathius, Hayman, and Merry and Riddell on α 173. Now it is regarded as evidence that Homeric Ithaka is S. Maura, connected with the mainland by a ferry (reckoned, says Dr. Leaf, "of course part of the land journey") at the end of an approach by land, and Dr. Leaf has a new reason to adduce. The land approach "was at least unusual and probably not quite respectable; at least it may have laid a newcomer under the suspicion of friendliness with the rough hill-tribes inland" (pp. 167 f.). But surely this is to read into the lines just what you want to find there. There is no warrant in the text. Take the first of the passages, α 170 ff., relating to the arrival of Athené-Mentes, and consider if there be any suspicion in Telemachus' mind. Not only is there no trace of it, but he seems only too eager to welcome the stranger. I need not quote the various expressions which show that. There is not the smallest reason for thinking the newcomer had arrived by a "not quite respectable way." And there is one thing in the formula that is fatal to the inference of the Leukadists, and that is that it starts by assuming in the clearest terms that the stranger has come by a ship, and not by a ferry-boat. That is evident from the second and third lines in the quotation given

above. He who puts the question assumes as a matter of course that the stranger has come in a *νηὺς* manned by regular *ναῦται*, and the inquirer even desires to know who the sailors are, that is, of what nationality. The alternative of a ferry is absolutely excluded, for in that case there would be no need of such questions.

That, I urge, is fatal to the Dörpfeldian view. The last line in the quotation is nothing but a primitive joke, "silly" it may seem to some of us now, but a joke nevertheless. As van Leeuwen notes on α 173, *verba urbane iocantis*. He refers to ἐμβαδὸν ἵζεσθαι ἥν πατρίδα γαῖαν, O 505, and, when I turn to Dr. Leaf's own commentary on that passage, I find the note, "ἐμβαδόν, on foot, a sarcastic taunt, reminding one of the artless humour of the words of Telemachus to the visitor in his island," and α 173 is referred to. The jokes in Homer are primitive to our modern taste, but they are there. Van Leeuwen refers also to τ 163, and might have added π 204, οὐ μὲν γάρ τοι ἔρ' ἄλλος ἐλεύσεται ἐνθάδ' Ὀδυσσεύς, and the Οὔτις affair. I could quote other passages, and also modern parallels from my own primitive country, Scotland. Parallels have also been given from Germany and modern Greece, and λ 158 f. is not to be forgotten. And if it be said, as it is, that the formula is not so appropriate in π 57-59 (Telemachus to Eumaeus) as in the other passages, the answer is that a scrutiny of the Homeric formulas shows that in many cases a particular form of words is used in a context in which it is not so appropriate as in other passages. Every Homeric student knows what havoc the critics have made through neglect to observe this.

Dr. Leaf also refers to the ferrying of cattle by the neatherd Philoitios, ν 185 ff., and declares (p. 153) that, if Thiaki is Ithaka, "it is not possible to believe that such a thing can have been done." I cannot think that anyone will hesitate to believe in the possibility after reading Mr. Allen's pages 92 and 98 f., with notes. Dr. Leaf has surely forgotten Noemon and his mares and mules in Elis, δ 634 ff., and Elis is much farther from Thiaki than Acarnania is. For the transportation to Thiaki in boats of several hundred swine from one of the islets between it and the mainland, see Mure's *Journal of a Tour in Greece*, I, 14. Dr. Leaf brings into the argument the squalls that infest this piece of sea, just as he used such "gusts of rugged wing that blow from off each beakéd promontory" for his Aulis

theory, on which see *CR*, XXXI, 7 ff. But good weather and a calm sea would not be altogether unknown. Noemon could manage in spite of squalls. Dr. Leaf appears to think of Philoitios as ferrying over his contributions to the Wooer's table daily. Others have thought the same, and have been refuted by Groeschl, Michael, and others. The *Odyssey* furnishes no ground for such a belief. The transportation of flocks by islanders to other islands or the mainland for grazing is known in other parts of the world. See Eustathius, 191, on B 145, for Ikaria and Samos. The practice prevails in the Aran Islands off the west coast of Ireland.

The supposed "telescoping process," the outcome of an imagined "thrust," by which the changes in the populations and names of these islands are assumed to have been caused, is discussed by Dr. Leaf on pages 154 f. It is the weakest point in a weak theory. Because, such changes have taken place elsewhere—on which see Mr. Allen, page 97—it "may well be," as Dr. Leaf puts it on page 53 of his book, that they have occurred in these Ionian Islands. The assumption is only an assumption, for there is no evidence to support it, and, more than that, it is against the tradition and the probabilities as these can be argued from the nature of the terrain and the geographical relations of western Greece. The tradition that exists regarding the coming of the Dorians must be disregarded, and the Dorians made to take a new route, in order to support a new hypothesis. It is amazing procedure. But I can refer for full discussions to Mr. Allen and others, as G. Lang, *Geographie der Odyssee*, 7, 97; Groeschl, *Dörpfelds Leukas-Ithaka Hypothesis*, 39 f.; and Paulatos, *Η ΠΑΤΡΙΣ ΤΟΥ ΟΔΥΣΣΕΩΣ*, 129 ff. What Dörpfeld "thinks" on this point, in order to complete his case, is stated by Seymour, *op. cit.*, 76 f., but Seymour abstains from approval of the "assumption." If Dörpfeld could make out a strong case in other respects, he might be forgiven for attempting the explanation he suggests, but there is hardly a piece of evidence for his theory that has not been refuted. On the point now under reference what answer is there to Mr. Allen's questions?—"If Thiaki-Ithaca were not really Ithaca, how did it get that position? Could an unimportant rock which in the classical period had no history except its heroic past, have appropriated Ulysses and Penelope? And was a

flourishing Corinthian colony, such as Leukas, to abdicate its rightful heroic past in favour of an unimportant rock?" There is no reply but this, that it is in the highest degree improbable, and that it could be believed only on the strongest evidence, and such evidence there is none. I would only add that the mere description of Ithaka, ν 242, οὐχ ἰππῆλατός ἐστιν, while Leukas is *von allen ionischen Inseln gradezu das Rosseland* (Engel, *Wohnsitz*, 23), and the omission, in the Homeric descriptions of Ithaka, of all reference to the white cliffs which are described as so conspicuous a feature of the scenery of Leukas, are enough to condemn Dörpfeld's hypothesis out of hand.

In short, I think Dr. Leaf's arguments in regard to Meges and Dulichium quite fail to convince. At any rate I close with the earnest hope that readers of *Homer and History* will not allow themselves to be deterred by the mere strength of its language from testing its conclusions for themselves. The pleasant style in which its author writes and the confident tone he assumes secured a favorable reception for the book, but when one sits down and examines carefully *ex Homero* the multitude of novel propositions which the new theory involves—some of them are enumerated in the *Classical Weekly*, XVII (1923), 22—the whole scheme proves to be unsubstantial.

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## PINDAR AND HOMER

BY EDWARD FITCH

Pindar uses the name Homer three times in his extant works. The question here proposed is: What did the name mean to Pindar? These three passages are primary sources of information. There is, besides, collateral evidence.

The first passage to be considered is in the fourth Pythian,<sup>1</sup> vss. 277 ff. τῶν δ' Ὀμήρου καὶ τότε συνθέμενος ῥῆμα πόρσυν' ἄγγελον ἐσλὸν ἔφα τίμειν μεγίσταν πράγματ' ἵπαντὶ φέρειν. This saying of Homer, "a good messenger brings greatest honor to every matter," is not far removed in sense from O 207: ἐσθλὸν καὶ τὸ τέτυκται, ὅτ' ἄγγελος αἴσιμα εἰδῆ. The scholiast says that this is the Homeric verse that Pindar has in mind. Beyond the ἄγγελον ἐσθλόν there is no close verbal likeness, and of the remaining words Pindar's πράγματι is post-Homeric. There is, then, no considerable part of a hexameter lurking below the surface, as in Ol. 6. 17. Professor Gildersleeve makes the comment: "We must remember that Homer was a wide term and Pindar may have had a bad memory." Our inquiry lies along the line of the first of these alternatives. How wide a term is "Homer" to Pindar? Does it exclude everything outside the *Iliad* and *Odyssey*?

The second passage is in the fourth Isthmian, dedicated to Melissus of Thebes. Pindar alludes (vss. 1-29) to the ups and downs of fortune which have come to the victor's family, and remarks upon the present distinction of Melissus, as the return of a fame that had been eclipsed. Fortune, he says, gives of both this and that, and the craft of lesser men has often overthrown the better. Then comes the illustration from the life of Ajax, the man of deeds, who was defeated in the contest for the arms of Achilles by the subtle Odysseus. ἵστε μὲν Αἴαντος ἀλκὰν φοῖνιον, τὰν ὀψία ἐν νυκτὶ ταμῶν περὶ φασγάνῳ, μομφὰν ἔχει παίδεσσιν Ἑλλάνων ὅσοι Τρῳάνδε ἔβαν. The bloody death of Ajax just before dawn, when he fell upon his own sword, driven

<sup>1</sup> References are to the edition of Sandys, Loeb Library.

to suicide by the deep feeling of the wrong that had been done to him, is one of the outstanding events of the Trojan story. The *Ajax* of Sophocles is evidence of this, so too the three passages in Pindar. Furthermore, the story already existed in poetical form. A definite point of contact between Pindar and his source is the phrase *ὀψία ἐν νυκτί*. The scholiast calls attention to the agreement: *ὁ γὰρ τὴν Ἀιθιοπίδα γράφων περὶ τὸν ὄρθρον φησὶ τὸν Ἀλάντα ἐαυτὸν ἀνελεῖν*. There is no single fact about the *Aethiopsis* more positively attested than that it contained the story of the death of Ajax, apparently told in a simpler form than that in the *Little Iliad*, that is, without the episode of the madness and the slaughter of the cattle. This simpler form is evidently the one in Pindar's mind.

The next following words after this reference to the death of Ajax are: *ἀλλ' Ὀμηρὸς τοι τετίμακεν δ' ἀνθρώπων, ὃς αὐτοῦ πᾶσαν ὀρθώσας ἀρετὰν κατὰ ῥάβδον ἔφρασεν θεσπεσίων ἐπέων λοιποῖς ἀθίρειν*. The main thread of the context is: Ajax was grievously wronged, but Homer has redressed the balance. If then Homer has made Ajax to be honored for all time by "erecting all his valor" *πᾶσαν ἀρετὰν ὀρθώσας*, may we limit the record of Ajax to what the *Iliad* contains, and exclude from it the famous stories of the *Aethiopsis*? In other words, are we, in interpreting Pindar, shut up to the narrower meaning of Homer, *Iliad* and *Odyssey*, or are we free to accept the wider meaning, that other poems passed as Homeric? Those who take the narrower view are quite within their rights in pointing to the fighting record of Ajax in the *Iliad*. This evidence is legitimate, but the point at issue is as to its being exhaustive. Pindar's use of Homer's name in immediate connection with the story of Ajax as the *Aethiopsis* gave it raises a presumption in favor of the wider meaning. This presumption is confirmed by Pindar's own words in another ode, the eighth Nemean.

This ode in honor of Deinias of Aegina brings the Aeginetan hero Ajax once again to the fore. The bane of envy is the theme that is illustrated by the fate of Ajax. "Envy smote Telamon's son," says Pindar, vs. 23, "by driving him upon his sword. Him who lacks words, though stout of heart, forgetfulness claims amid baleful strife. The greatest prize falls to cunning falsehood. For by secret votes the Greeks favored Odysseus, while Ajax deprived of golden weapons



wrestled with death. Yet the wounds which they dealt the warm flesh of their foes in battle were by no means equal." Then follow the significant words: τὰ μὲν ἄμφ' Ἀχιλεὶ νεοκτόνῳ, ἄλλων τε μόχθων ἐν πολυφθόροις ἀμέραις. It appears, then, that the greatest day of battle was that over the body of Achilles. So it was for Odysseus, too (ε 308-10). On that greatest of days Ajax was the better warrior. For Pindar the mightiest of the deeds of Ajax fell outside the limits of what we now call Homer. In the light of the eighth Nemean, the πᾶσαν ἀρετάν of the fourth Isthmian, whatever it includes, may not exclude the fight over the body of Achilles.

The third occurrence of the name Homer is in the seventh Nemean. Once again, in this context, is found the story of the death of Ajax. Pindar is claiming for the poets true discernment, unblinded by gain. "I for my part," he says (vs. 20), "believe that Odysseus got more than his deserts at the hands of Homer. Upon his fictions and his winged skill there rests something impressive. The poet's art steals away our hearts, beguiling us with song. Blind of heart is the mass of mankind." This general sentiment concerning the incapacity of men to judge with discernment Pindar has placed between the preceding reference to Homer and the following reference to Ajax. The poet continues: "Had men been able to see the truth, the mighty Ajax, in wrath for the arms, would not have thrust the sword through his heart; Ajax, the mightiest in battle after Achilles." The connection in thought in the passage as a whole seems clear: first a concrete reference to Homer's power of creating illusions; then a maxim about the blindness of men; and last a concrete case in point, namely the blind decision of the judges which drove Ajax to his death. The two concrete cases lie on either side of the maxim. They face each other, in heraldic fashion as it were. Homer and the death of Ajax are once more an associated pair, as in the fourth Isthmian.

The passages thus far considered yield this definite result, that for Pindar Ajax and Odysseus form a contrasted pair. They are rivals and opposites. But in the *Iliad* and *Odyssey* they do not in any adequate sense constitute such a pair. The wrestling match, Ψ 700 ff., is too slight an incident to be the explanation of the relation in which these two heroes stand in Pindar's world. The mention

of the quarrel over the arms in λ 541-62 is not an independent version.<sup>1</sup> It assumes a well-known story. The story itself occurred in two poems of the Cycle, the *Aethiopis* and the *Little Iliad*, with the former of which Pindar's references seem to be in agreement. When Pindar mentions Homer's name in connection with this episode we fairly conclude that his meaning of Homer was the wider, not the narrower-meaning.

There is collateral evidence that leads to the same conclusion. In Aelian's *Varia Historia* 9. 15 is a brief reference to the high esteem in which the Argives hold Homer. When they sacrificed they were wont to summon Apollo and Homer. Then follows a second story, introduced by λέγεται δὲ κάκεινο. This story, that Homer for lack of a dowry for his daughter gave her the *Cypria*, is quite distinct from the first, and this is not the only record of it. In the *Lexicon* of Suidas, under "Ὅμηρος" (Allen, *Homer V*, p. 258, 35-36) is mention of Homer's marriage and of his two sons and "one daughter, whom Stasinus the Cyprian married." This supplements the brief reference of Aelian and is in fact pointless unless the story of the dowry be assumed. Suidas further says (ll. 37-38) that the undisputed works of Homer are the *Iliad* and *Odyssey*, but adds that other poems are attributed to him, among them the *Cypria*. The value of this total record is clear. The *Cypria* was once attributed to Homer. It was also attributed to Stasinus. The two conflicting stories are harmonized into the composite form according to which the poem becomes a dowry given by Homer with his daughter. This harmonized version appears in the *Chrestomathy* of Proclus (Allen, *ibid.*, p. 97, 12-17). Two witnesses then, appear to vouch for the existence of this version about the dowry. Aelian agrees with them, in the main, omitting the name Stasinus but adding the important item καὶ ὁμολογεῖ τοῦτο Πίνδαρος.

The validity of Aelian's testimony has been called in question by Professor Scott (*Homeric Unity*, 25-26) on the ground that the important phrase about Pindar may not with certainty be referred to the story about the dowry. A candid reading of the whole passage, however, shows that there are two main topics, and that with λέγεται

<sup>1</sup> Cf. Jebb, *Introd. to Soph. Ajaz*, xii.

δὲ κακῆϊνο the author definitely and clearly passes to the second topic. To this second topic the words in question belong. Furthermore, to deny the value of what Aelian mediates because the intermediary is a late compiler is a counsel of despair. The decisive consideration is not Aelian himself but the quality of his sources. Wheat is wheat, even in a bushel of chaff. For example, the didascalic record of the *Troades* of Euripides is not to be discredited because it is preserved by Aelian, but it is to be credited because of the known character of the documents of its sort. So too this statement concerning Pindar and the *Cypria* is to be taken in connection with all other available material.

The view that Pindar accepted the *Cypria* as Homeric can be further supported. The odes of Pindar, when compared with the *Cypria*, give this confirmation. The *Cypria* has perished, but the limits within which the action took place and the main events are known beyond doubt. The poem narrated the antecedent circumstances of the Trojan War, and the war itself down to the *Iliad*. Now it appears from the odes that Pindar knew these stories of the *Cypria* and that they meant much to him. In the ninth Olympian 70 ff., wishing to celebrate Patroclus, he recalls not the *Iliad* but the exploit of Achilles in opposing Telephus, when Patroclus appeared as friend and squire of Achilles. Telephus, king of Mysia, ally of the Trojans, was a prominent figure in the *Cypria*. In the fifth Isthmian, 39 ff., the wounding of Telephus is reckoned as one of the four great exploits of Achilles. In the eighth Isthmian, 50, Telephus stands first in the swift rehearsal of what Achilles performed in war. Pindar's Achilles is not easily understood if one holds with Professor Scott (*Homeric Unity*, 32) that "the poetry of the Cycle was generally despised and neglected." Certainly it was not despised by Pindar. In the eighth Isthmian, 50 ff., Telephus, Memnon, and Hector are cited as proofs of the prowess of Achilles. In the second Olympian, 81-83, the list is Hector, Cynus, and Memnon. In the third Nemean, 63, and the sixth, 51 ff., Memnon alone is named. In the fifth Isthmian, 39 ff., already referred to, the list includes, besides Telephus, Cynus, Hector, and Memnon. The canvas of the *Iliad* is too narrow for Pindar's Achilles. He overlaps on both sides;

witness Cynus and Telephus for the *Cypria* and Memnon for the *Aethiopis*. Cynus is an unmistakable sign of the *Cypria*. Even in Theocritus (16. 49) he retains his place among the "Homeric" heroes. Then there is the sixth Pythian, with its famous picture of the filial Antilochus who saves his father's life at the expense of his own. This story points back to the *Aethiopis*. The tenth Nemean, 54-90, recounts the death of Castor and the choice of Polydeuces not to be parted from his mortal brother. Pindar is here following the *Cypria*, as Aristarchus recognized when he advocated a reading which would bring Pindar's text into harmony with the *Cypria*. More than five verses of the poem are quoted by the scholiast. Two events that frame in the life of Achilles have especial prominence in Pindar's odes. His death was mourned by the Greeks (Pyth. 3. 101-3) and by a choir of Muses (Isth. 8. 57-60). He dwelt after death in the gleaming island in the Euxine Sea (Nem. 4. 49). These particulars are specified by Proclus in his summary of the *Aethiopis*. The other event is the marriage of Peleus and Thetis, where the gods were guests and brought gifts (Pyth. 3. 88 ff.; Nem. 4. 65 ff.). The song of Apollo and the Muses at the wedding is mentioned in Pyth. 3. 90, and is elaborately reported in Nem. 5. 22 ff. The wedding of Peleus and Thetis as an important episode in the *Cypria* is abundantly attested. Pindar gives it prominence. In fact, his emphasis is like that of the François vase, which has as its dominant piece of decoration the belt depicting the wedding procession, and below it another famous scene from the *Cypria*, the death of Troilus, while two lesser scenes show the influence of the *Iliad*. Whatever may have been true in later times, it is plainly not true for Pindar that the poetry of the Cycle was despised and neglected.

To return now to Aelian, who says that Pindar shared the view that the *Cypria* was given by Homer as a dowry for his daughter: the credibility of this testimony is put beyond reasonable doubt by this evidence of the esteem in which Pindar held the *Cypria*. While one cannot say that in all details Pindar's version is that of the *Cypria*, the *Cypria* is nevertheless a great background against which Pindar works. There is then no valid reason to doubt that Pindar shared the wider view of the meaning of "Homer." Why hesitate

to accept such evidence concerning the reverent Pindar when the sceptical Thucydides quotes the *Hymn to Apollo* as Homer's?

The general course of ancient opinion concerning the meaning of the term "Homer" and the authorship of various poems is conveniently summed up in the *Chrestomathy* of Proclus (Allen, V, p. 102, 1-6): γέγραφε δὲ ποιήσεις δύο, Ἰλιάδα καὶ Ὀδύσειαν, ἣν Ξένων καὶ Ἑλλάνικος ἀφαιροῦνται αὐτοῦ. οἱ μέντοι γε ἀρχαῖοι καὶ τὸν κύκλον ἀναφέρουσιν εἰς αὐτόν. προστιθέασι δὲ αὐτῷ καὶ παίγνια τινα, Μαργίτην, βατραχομαχίαν ἢ μυομαχίαν· ἐπτάπεκτον αἶγα, κέρκωπας κενούς. This passage is ultimately of value because it can be verified. The underlying assumption of the passage is that the ascription of the *Iliad* and *Odyssey* alone to Homer is the result of a critical judgment, working in a limiting way on a larger mass of material. The criticism of Xenon and Hellanicus who went to the point of separating the *Iliad* and *Odyssey* did not prevail in antiquity. The process stopped at the earlier point, leaving to Homer two poems only. This result, then, was reached in antiquity through a process. This process, since it is in part a vital as well as a critical one, does not go forward evenly. It is possible to discern its beginning in Herodotus<sup>1</sup> and its completion in the Alexandrian criticism. But no hard and fast line can be run. Aristotle had clarified his views concerning the Cycle, yet shows an unclarified remainder in his remark about the *Margites*. Callinus, Aeschylus, Thucydides, held the wider view. These are important ones of the ἀρχαῖοι who along with Pindar had no canonical Homer. Local pride sometimes favored the persistence of the older view. The Athenian tribe Acamantis was named from the hero Acamas, son of Theseus and grandson of Aethra. In the *Funeral Oration* ascribed to Demosthenes, there is a rehearsal of the noble traditions concerning the ten eponymous heroes of the Athenian tribes. Concerning Acamas the following is said, c. 29: ἐμέμνηντ' Ἀκαμαντίδαι τῶν ἐπ' ὧν ἐν οἷς Ὅμηρος εἶνεκα τῆς μητρὸς φησιν Αἰθρας Ἀκάμαντ' εἰς Τροίαν στείλαι. Acamas is unknown to the *Iliad* and *Odyssey*, but appeared in the *Iliu Persis*. Similar survivals of the earlier and wider view appear after the time of Aristotle. Antigonius of Carystus<sup>2</sup> is a case in point, who classes together the *Odyssey*, the

<sup>1</sup> Mure, *History of Greek Literature*, I, 183.

<sup>2</sup> *Classical Journal*, XVII, p. 94.

*Hermes Hymn*, and the *Thebais* as Homeric. The passage in Xenophon's *Symposium* to which Professor Scott appeals (*Homeric Unity*, 31) to prove that "all the verses of Homer" and "*Iliad* and *Odyssey*" are interchangeable terms, even if it be so interpreted merely registers a certain advanced point in the critical process which ultimately issued in a result universally accepted. But we may not in any case impose Xenophon's horizon upon Pindar. Pindar and Herodotus bear comparison. Like a positive and a negative pole, they complete a circuit. Herodotus denies; his denial is made intelligible by what Pindar believes. Between Pindar and Xenophon is Thucydides (III, 104), who quotes five verses of the *Hymn to Apollo*, with the preface *δηλοῖ δὲ μάλιστα Ὅμηρος*, then adds eight more verses from the same hymn with a concluding remark, that seals his allegiance to the wider view: *τοσαῦτα μὲν Ὅμηρος ἐτεκμηρίωσεν*.

Professor Scott believes in the unity of Homer, that is, that Homer composed both the *Iliad* and the *Odyssey*. Further, he seeks to support his view by adducing external evidence to the effect that in antiquity no other works were ever ascribed to Homer. Homer was, he holds, from the beginning a term of constant value. Over against this view of Homer there is a mass of evidence that refuses to be dissipated. It is, however, susceptible of being understood. Considered as a series of glimpses into a process that gradually issued in a result, it is perfectly intelligible. The result is the judgment which was finally reached in antiquity that the *Iliad* and *Odyssey* are Homer's, and that no others are. This result can be sufficiently explained on the theory that in antiquity when this judgment was being formed internal evidence was the only available basis of judgment. Was there, or is there, in addition external evidence such as Professor Scott seeks? In answer one must say that much of what purports to be external evidence concerning Homer is nothing more than inference from the poetry itself. This is plainly true of Herodotus and Thucydides. They did not know, they inferred. The belief that Homer was blind cannot free itself from the suspicion of being an inference merely, rather than a fact handed down. In the second *Life* of Homer, ascribed to Plutarch, there is preserved the statement that Pindar called Homer both a Chian and a Smyrnaean. This sounds like a tradition. But if we assume that such



fragments of genuine tradition exist in the extant *Lives*, side by side with mere guesses, we must at the same time recognize that the exclusive view of what is genuinely Homeric is no part of this biographical tradition. On the contrary, the inclusive or wider view repeatedly appears. Of this wider view Pindar is an outstanding example. To say that Pindar knew and followed the *Cypria* and held the *Cypria* to be Homeric is not to affirm that the *Cypria* was as great as the *Iliad*, or that Pindar was undiscerning. It is merely to recognize that the judgment of Aristotle (*Poetics*, ch. 23) had two aspects, a reverse as well as an obverse. The *Iliad* and *Odyssey* are distinguished from the poems of the Cycle by having unity and artistic form. This is Aristotle's main statement. But he also recognizes that the poems of the Cycle were great storehouses of material. Our evidence shows that Aeschylus and Pindar used them as such, and used them freely and thankfully. What they did agrees well with the note of appreciation that is heard in the saying about Sophocles (Athenaeus, V, 277E) that he rejoiced in the Epic Cycle and got whole dramas out of it. Aeschylus, Pindar, and Sophocles belonged to the pre-critical and pre-canonical period. We attribute to them the temper of a later age if we insist that they thought canonically about Homer.

HAMILTON COLLEGE

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## PROFESSOR BASIL LANNEAU GILDERSLEEVE

BORN OCTOBER 23, 1831—DIED JANUARY 9, 1924

Two sentences which Professor Gildersleeve spoke in the presence of his students contain the essence of his career: "Every mistake that can be avoided by patient labor is a sin," and "It is hard to sit in the seat of the scornful of petty grammatical distinctions."

His unflagging efforts to master the most hidden facts, even though of seemingly small importance, and his horror of the most trifling errors made his life a challenge to the best in contemporary scholarship. He never trusted to his own shrewdness to reach the truth, but he patiently assembled and compared the facts, then revised and verified what he had done. This trait of industry was his life, and, after he had retired from teaching, a friend urged him to be less active and to enjoy a long-earned rest, but he replied, "It has been wisely said that every man is as indolent as he dares, and I was born a coward."

He believed that the greatest contribution the Greeks had made to civilization was their language, and that this language was the supreme expression of their wonderful sense of beauty. He deliberately set for his life's work the discovery and the interpretation of the beauties hidden in the kaleidoscopic and delicate shiftings of cases, moods, and tenses.

In his teaching and interpretation of various authors he rarely quoted literary parallels, or referred to history, or antiquities, but he confined himself almost exclusively to bringing out the secrets hidden in the language.

The joy he found in these minute studies kept him young in spirit, and he never became a recluse or the absent-minded professor; no stories of absurd and forgetful acts were ever attached to his name. His greatness of mind and of character was so evident to all that he was held in reverent esteem even by those entirely ignorant of his special field.

If the "mind is the measure of the man" and the growth of that mind the best of all growths, its harvest the richest of all harvests, then Professor Gildersleeve must be given a place among the very greatest men America has produced.

When the length of his career is considered and its unswerving devotion to the highest ideals there are but few in all the ages to compare with him.

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JOHN A. SCOTT

## NOTES AND DISCUSSIONS

### NOTES ON ATHENIAN CHRONOLOGY

The chronology of Athenian history in the generation preceding the Chremonidean War is still a problem awaiting a final and conclusive solution. Mr. Tarn has recently shown that the discovery of new inscriptions may result in the overthrow of any list of archons based upon preconceived theories, for positive evidence is still lacking for this obscure period.<sup>1</sup> The net result of Tarn's article is to establish definitely the fact that there was a Nationalist government in Athens in 279 and in the archonship of Telokles. But, since I do not agree with his interpretation of the history of Athens following the death of Lysimachos, I propose to examine the evidence anew in the hope that fresh discussion may advance the solution of this vexed problem in some degree.

I shall take as my starting-point the fragment of Philodemos which Mayer<sup>2</sup> has restored as follows: ἀ[πο]ρ[έ]φαντος [αὐτὸν] τοῦ μ[ι]ᾶν λαμβάνειν [τ]ὴν βασιλείαν καὶ μετὰ τὴν Λυσιμάχου τελευ[τ]ὴν παρ[έ]χ[οντ'] αἱ σπονδ[α]ὶ <τῷ> Γονατῇ καὶ [Δακόν]ων κρατήσας τῆς Μακε[δο]νίας ἐκπ[ί]πτει πάλιν εἰς τὴν Ἀσίαν, εἰθ' ὕστερον ἔ[τε]σιν ᾗ νικήσας Κελ[τοὺς] τοῦ βα[σι]λ[ε]ύειν Μακεδόνων ἤρξατο, κτλ. From this we learn that after the death of Lysimachos, Antigonos made a treaty with someone. The problem is to determine the other party. It could not have been Ptolemy Keraunos, Seleukos, Antiochos, or Ptolemy Philadelphos. The latter is out of the question. The others were all claimants of the throne of Macedon, to which Antigonos himself laid claim. Seleukos himself was killed by Ptolemy Keraunos after crossing into Europe to claim the kingdom of Lysimachos. Memnon and Justin give ample evidence that there was a three-cornered struggle between Keraunos, Antigonos, and Antiochos.<sup>3</sup> After the defeat of Antigonos by Keraunos the latter concluded peace with Antiochos and won over Pyrrhos—or at least gained his neutrality by giving his daughter in marriage to him.<sup>4</sup> To return to the truce to which Antigonos was a party, we believe that it could have been none other than the famous secret treaty concluded with Pyrrhos.<sup>5</sup> The latter was preparing to set forth on his great adventure for founding a western empire, and was willing to stipulate that Antigonos should support him with troops when called upon, in return for the acknowledgment of his

<sup>1</sup> *J.H.S.*, XI (1920), 143 ff.

<sup>2</sup> *Philologus*, LXXI (1912), 226. The first line is somewhat of a puzzle. I suggest the following: τοῦ μ[ι]ᾶ α[ν]δ[ρ]α λαμβάνειν κ.τ.λ.

<sup>3</sup> Justin XXIV. 1, 1 (cf. Trogus, *Prologus* XXIV); Memnon XVIII.

<sup>4</sup> Justin XXIV. 1, 8.

<sup>5</sup> Tarn, *Antigonos Gonatas*, pp. 115–16, n. 8.

claims to Greece or Macedon which had now ceased to interest the Epirote king. Antigonos failed to keep his obligations with his ally—whether because he had not forgotten the treaty concluded between Pyrrhos and Keraunos, or because he was in no position to furnish the troops when the demand came, is immaterial. We know the vengeance which Pyrrhos took on his return, and Antigonos lost his kingdom again.

Philodemos goes on to say that after the treaty Antigonos got possession of a state whose name Mayer restores as [Λακό]νων. Tarn rightly objects to this restoration. I had formerly suggested [Μακεδό]νων, but I now believe the correct restoration is [Ἀθη]νῶν. That is, Antigonos recovered Athens and the Peiraeus after the death of Lysimachos and his treaty with Pyrrhos. Tarn is certainly correct in his interpretation of *IG. II.*<sup>2</sup> 666–67 when he claims that there were two wars.<sup>1</sup> Strobichos aided in the recovery of the Museion when he first deserted Demetrios. The second war is that in which Antigonos sought to recover Athens in the archonship of Menekles. His first attempt was a failure and he withdrew to Boiotia. In the spring he returned unexpectedly with his army and as the Athenians were short of grain they surrendered at discretion.<sup>2</sup> Phaidros was elected agonothele and celebrated the games in the early summer of 280.<sup>3</sup> By chance it happened that the secretaryship fell to the tribe Antigonis for the year 280–279 and there is no likelihood that the pro-Macedonian government disturbed the cycle when it came into power at this time.

Events now moved swiftly. Seleukos was assassinated early in 280. The exact date is unknown beyond the fact that it was seven months after the defeat of Lysimachos at Koroupedion.<sup>4</sup> Keraunos seized the kingdom of Macedon and both Antigonos and Antiochos prepared independently to press their own claims. The movements of Antiochos were hindered by revolts in his own kingdom, and Keraunos had much of his father's army in Macedon.<sup>5</sup> Antigonos gathered a fleet and an army for a double assault. His fleet was defeated decisively, and he deemed it wiser to withdraw his army.<sup>6</sup> Philodemos says he was driven out of Macedon into Asia,<sup>7</sup> while Memnon says that he withdrew to Boiotia. These statements are apparently contradictory, but I think that they can be reconciled. Justin states that almost all the Greek cities rose against Antigonos on the news of his defeat.<sup>8</sup> Athens certainly was Nationalist in Boedromion in 280 because the Board of Administration was in power at that time.<sup>9</sup> We also know that

<sup>1</sup> *J.H.S.*, XL (1920), 145.

<sup>2</sup> The course of events is narrated by Tarn, *Antigonos Gonatas*, pp. 125 ff., but he places them before the death of Lysimachos.

<sup>3</sup> *IG. II.*<sup>2</sup> 682. <sup>4</sup> Justin XVII. 2, 3. <sup>5</sup> Memnon XV–XVII. <sup>6</sup> Memnon XIII.

<sup>7</sup> Philodemos says that Antigonos was driven out *again*. He apparently means that Antigonos regarded himself as king of Macedon *de jure* from the archonship of Euthios. There is no evidence that he made a previous attempt to claim his inheritance.

<sup>8</sup> Justin XXIV. 1. 2–3.

<sup>9</sup> *IG. II.*<sup>2</sup> 665–67.

Phaidros was replaced by Glaukon as agonothele in the course of the year.<sup>1</sup> Therefore, Antigonos finding the cities closed to him on the mainland decided suddenly that his best venture was to recover the possessions of his father in Asia rather than to waste time in Greece. Antiochos was evidently in trouble and I suspect that Nicomedes offered an alliance in return for his assistance.<sup>2</sup> Antigonos accordingly crossed immediately to Asia from Boiotia or Chalkis and the scene of his activities for the next few years shifts to that quarter.

I believe that all the evidence can be reconciled and fitted together—both literary and epigraphical—if we accept this chronology for the events between 282 and 279. The arrangement of the cycle of secretaries by Kolbe and Tarn is open to the fatal objection that they are compelled to assume breaks in the official order which cannot be explained. In my opinion there could have been no break under the Nationalist government. Nor is it likely that the Macedonian party would have passed over the tribe Antigonis, which was due to hold the secretaryship when they recovered Athens. A more important question arises about the policy of the Nationalists when they regained control of the political machinery in the fall of 280. They changed the agonothele—an elected officer. Did they change the secretary and drop the tribes Antigonis and Demetrias? If they did, then the cycle which I advocated formerly must be brought into conformity with that of Ferguson, Kirchner, and Tarn. Unfortunately, I do not think that any change was made by the Nationalists in the offices to which appointments were made by lot and they continued the cycle without a break.

By proving that Telokles was archon under a Nationalist government, Tarn has injected a new element into the problem of the chronology of the archons and we must now consider the date of this magistrate. From 289 to 262 there are three different arrangements of the cycle, Pomtow-Kolbe,<sup>3</sup> Ferguson-Kirchner-Tarn,<sup>4</sup> and that which I advocated some years ago.<sup>5</sup> The problem resolves itself into a question of the establishment of the Soteria at Delphi which is placed by the three different theories in 276, 274, and 272, respectively. Unfortunately, there is not sufficient evidence to decide this point conclusively.<sup>6</sup> I believe, however, that the theory of Pomtow and Kolbe must be rejected, not only because of the inexplicable breaks in the

<sup>1</sup> Ditt. *Syll.*<sup>3</sup> 365. To ascribe this document to 296 gives Glaukon an unusual length of public service and seems improbable.

<sup>2</sup> Memnon XVIII. <sup>3</sup> *Philologus*, 74 (1917), 58 ff. *Hermes*, LI (1916), 530 ff.

<sup>4</sup> Ferguson, *Priests of Asklepios*; Tarn, *J.H.S.*, XL (1920), 143 ff.

<sup>5</sup> *Classical Philology*, IX (1914), 248 ff.

<sup>6</sup> The Chian decree (Ditt. *Syll.*<sup>3</sup> 402) is unfortunately broken at the critical point. The delegation to the Soteria is to be chosen when the *theoroi* were elected for the Pythia or the Olympic festival. I prefer Pomtow's restoration as [καθιστ]ῶνται is not the word we should expect for the choosing of ambassadors. But as the inscription is not *stoichedon* the lines may vary considerably in length, and no sure argument can be based on the length of the line. If we could determine whether the Soteria were celebrated in midsummer or after the Pythia, our problem would be simplified.

cycle which they are compelled to assume, but also because it seems impossible to place the Nationalist archon Glaukippos before Peithidemos in whose year the Chremonidean War broke out. Tarn has also clearly shown that the date of Polyeuktos in 277/6 is impossible.<sup>1</sup>

Had we a few more secretaries in the obscure period following 279 we could easily solve the problem of the cycle, but at present we lack the evidence for a conclusive solution. In spite of Tarn's objections I still believe that Philokrates officiated as archon in a war year. Whether Tenedos was under the Ptolemies or Antiochos may be disputed. Ptolemy, however, controlled most of the Aegean even as far north as Samothrake, and I can see no objection to the assumption that Tenedos was also under his control, but nothing can be proved from this point in view of the uncertainty.<sup>2</sup> More important, however, are the decrees from Athens and Eleusis praising the military officials of that year. An Athenian hipparch was elected to command the forces at Eleusis.<sup>3</sup> The Athenians had an army officered, not by mercenaries but by their own citizens. Now Antigonos could not have been blind to the intrigues of Egypt, and I doubt very much if he would have permitted so much freedom to a subject state at such a time. It is true that Athenians officered the troops in various stations throughout Attica at a later time, but the conditions were very different.<sup>4</sup>

Another point might be urged in favor of the cycle as we have constructed it. In the archonship of Euboulos, Neoptolemos was secretary. Aigeis had performed its duties as prytanizing tribe with particular distinction. In a fragmentary decree which is preserved the tribe was honored by the city, and the members of the tribe crowned various members of the tribe who had held office.<sup>5</sup> Since Eukles who was also crowned was not a member of Aigeis, it is possible that Neoptolemos belonged to a different tribe. According to our arrangement of the cycle, Euboulos had a secretary from Aigeis. It is tempting to take this inscription as proof that Neoptolemos was a member of Aigeis, but experience has taught me that it is unwise to build too much on such a tenuous argument.

The problem of dating Telokles still remains. Since he belongs in a Nationalist year, the two possibilities are 282/1 and 277/6. I prefer to leave Sosistratos in 282/1, although I believe that it is no longer possible to restore this name in *IG. II*<sup>2</sup>. 673. Since Anaxikrates is surely National, the latter document can best be dated in his year, although the other alternative is still possible. Telokles I would place in 277/6, and . . . λαος should go to 275/4.

From *IG. II*<sup>2</sup>. 700 we learn that the archon preceding Thymochares had ten letters in the genitive (if we restore *διέμεναν*) or nine (if we restore *διε-*

<sup>1</sup> *Loc. cit.*, 155 ff.

<sup>2</sup> Ferguson, *Hellenistic Athens*, 178; Tarn, *Antigonos Gonatas*, pp. 105 ff.

<sup>3</sup> *IG. II*<sup>2</sup>. 685, 1279.

<sup>4</sup> *Ibid.* 1286, 1287, 1299.

<sup>5</sup> *Ibid.* 678. It may be noted that *IG. II*. 835 cannot be assigned to the archonship of Euboulos as Kleigenes is secretary in the latter document.

τέλειαν). Either alternative is possible, or since the lines of this inscription are not of uniform length, there may be a lacuna at the end of line 10 and a shorter name may be permitted. There is no objection to placing Thymochares immediately after . . . λαϊος or in 274/3.

Tarn is right in maintaining 285/4 for the date of Euthios against Kolbe who dates this archon a year later, but his theory of two breaks in the cycle in the two successive years is untenable. I am also inclined to accept his chronology of the Gallic invasion, placing both invasions—that of Macedon and of Delphi—in the same season, although I think that something is to be said for my former view still. There is no evidence for the theory that there was a Nationalist government at Athens between 273 and 271 and, until satisfactory proof is forthcoming, I prefer to believe that Athens remained under the control of Antigonos from 277/6 until the outbreak of the Chremonidean War.<sup>1</sup> Possibly we should leave the question of the secretary-cycle between 279 and 262 *sub iudice*, but I believe that there was no break in the tribal order in 280, and that it was continued unbroken until the end of the war in 262. For convenience of reference we give our arrangement of the archons between 288 and 262 and the periods during which the Board and the Minister of Administration held office:

## οἱ ἐπὶ τῇ διοικήσει

288/7	Diokles IV
287/6	Diotimos V
286/5	Isaios 6
285/4	Euthios VII
284/3	Gorgias 8
283/2	Ouriros IX
282/1	Sosistratos? 10
281/0	Menekles XI

## ὁ ἐπὶ τῇ διοικήσει

(ca. March–August, 280)

## οἱ ἐπὶ τῇ διοικήσει

(from September, 280)	
280/79	Nikias Otryneus XII
279/8	Anaxikrates 1
278/7	Demokles 2
277/6	Telokles? 3

## ὁ ἐπὶ τῇ διοικήσει

276/5	Euboulos 4
275/4	. . . . . laios 5
274/3	Thymochares 6
273/2	Polyeuktos VII
272/1	Hieron VIII
271/0	Pytharatos 9
270/69	Philoneos 10
269/8	Theophilos XI
268/7	IG. II. <sup>2</sup> 702–3, XII

## οἱ ἐπὶ τῇ διοικήσει

267/6	Peithidemos 1
266/5	Philokrates II
265/4	Lysitheidēs <sup>2</sup> 3
264/3	Diognetos 4
263/2	Glaukippos V
262/1	Antipatros 6
	Arrheneides 1

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<sup>1</sup> *Classical Philology*, IX (1914), 248 ff.; *A.J.P.* XXXVI (1915), 424 ff.

<sup>2</sup> We now know that Lysitheidēs and Hieron were reasonably close together (*IG. II.*<sup>2</sup> 1317b). I believe that the name of the former should be restored in *IG. II.*<sup>2</sup> 689, and dated in 265/4 under a Nationalist government. Arrheneides belongs to the Macedonian period and the Board of Administration was replaced by a single official in his archonship (*Diog. Laert.* vii. 10).



## AN EMENDATION IN LUCIAN'S SYRIAN GODDESS

In his *Syrian Goddess*,<sup>1</sup> c. 29, Lucian gives a brief but highly interesting account of a pagan predecessor of St. Simeon Stylites. Each year, he tells us, a man ascended one of the tall phallic pillars in front of the temple of Atargatis at Hieropolis and remained there for seven days, praying for blessings on behalf of all Syria; also on behalf of particular individuals, who obtained special prayers by making a contribution at the foot of the pillar and giving their names, which were then called up to the man on top. In the passage of which that is the general purport, there is in the accepted text the following sentence: πολλοὶ δὲ ἀπικνεόμενοι χρυσὸν τε καὶ ἀργυρον, οἱ δὲ χαλκόν, κομίζουσιν, εἴτ' ἀφέντες ἐκείνου πρόσθε κείμενα ἀπ᾿ αἰσι λέγοντες τὰ οὐνόματα ἕκαστος. "Many come and bring gold and silver, or perhaps bronze; then, leaving them lying in front of him (or the pillar?), they go away saying each his name."

This is the reading of the fifteenth-century codex N (Par. 2957) and its fellows; it was introduced into the text of the second edition, the first Aldine of 1503, along with a good many other readings peculiar to that group of codices. Some of them improved the text, but others, and among them this one, did not. The reading of the first edition that it displaced was that of the par nobile fratrum, Γ (Vat. 90) and E (Harl. 5694), of the first quarter of the tenth century, which ran: πολλοὶ δὲ ἀπικνεόμενοι χρυσὸν τε καὶ ἀργυρον, οἱ δὲ χαλκόν, τὰ νομίζουσιν, ἐς ἐκείνου πρόσθε κείμενον κατ᾿ αἰσι λέγοντες τὰ οὐνόματα ἕκαστος. At first glance this is puzzling, and clearly corrupt; but careful reading will show that the corruption is very limited. We can get nearly the full sense of the words just as they stand. "Many, as they arrive, deposit (κατ᾿ αἰσι) gold and silver, or perhaps bronze, that they use for money (τὰ νομίζουσιν) in (something) placed in front, saying each his name." Though the phrase τὰ νομίζουσιν is somewhat odd and perhaps not quite correct, it is not, I think, corrupt. The relative is in the plural to include "gold and silver" as well as "bronze," and with a view to generalization; it is in the accusative because Lucian, who is imitating Herodotus, knows that he uses the accusative not infrequently with νομίζειν in a sense akin to χρῆσθαι; e.g., 1. 142. 1, 2. 42. 2, 4. 183. 2. All that seems questionable is the evident employment of that verb here to mean "use as money." This, I suspect, is an Atticism: but the slip, if indeed it is a slip, is of a sort perfectly natural in imitative writing and common enough elsewhere in this piece. Nothing else in the tradition, certainly, is at fault save the single word ἐκείνον.

This, beyond peradventure, is the legitimate tradition. The other did not give birth to it, but was illegitimately begotten upon it by a Byzantine corrector. Similar pieces of his patchwork are frequent in this group of

<sup>1</sup> On the authenticity of this piece see Allinson, *AJP*, VII, 206-S, and Penick, *Studies in Honor of B. L. Gildersleeve*, pp. 387-93. Further evidence, which I believe to be conclusive, will be presented soon in connection with a discussion of the *Astrology*.

MSS, though not often so bold and ingenious. It was no mean achievement to make some seven changes in a few words and at the same time manage not only to retain most of the original fabric but to call forth a meaning which, though it bore but the vaguest resemblance to the original, yet was sufficiently adequate to the general context to be in some degree plausible. To be sure, it will not stand close comparison with the original. His κομίζουσιν is not half bad; but νομίζουσιν, in addition to being the *difficilior lectio*, is the better one, since presumably the contributions were coin, not plate or bullion. The excision of τὰ and its subsequent reinsertion, transmogrified into εἶτα, is amusingly modern, but the resultant combination is wooden, and the word εἶτα itself would have been wholly unacceptable to Lucian in this piece as an Atticism. Both ἀφέντες (which should not have been left for Bekker and Dindorf to correct into ἀπέντες) and ἀπίασι derive from κατιάσι and embalm the corrector's uncertainty as to whether that form came from ἱημι or εἶμι. He asks us, moreover, to believe that the worshipers left their offerings lying about with a gesture of careless indifference and departed in hot haste, giving their names over their shoulder; but since we have an option in the matter, we must prefer, as more natural, the representation that they deposited them in something or other and gave their names as they did so, after which, for aught said to the contrary, they were free to wait about and get their money's worth if they wished. It is also significant that when he might perfectly well, or even better, have left κείμενον as it stood, he altered it into the plural simply because he could not forget the τὰ that he had excised! The most noteworthy point, however, is that although by changing ἐκείνον into the genitive he was able to make it depend on πρόσθε, he could not produce thereby a wholly satisfactory result; for the pillar (φαλλός) is too remote for the pronoun, and the man on top of it, since it is fabulously high, too remote for πρόσθε. Translators mirror the difficulty; "in conspectu eius," say Gesner and Dindorf; "unten an dem Phallus, worauf jener sitzt," Wieland. It is the original fault showing through.

To restore what Lucian wrote we have only to fetch up that ancient reading out of the footnotes and let it correct itself. The sense demands that ἐκείνον be a receptacle wherein money can be deposited; it must be converted into ἐχίνον, a pot or kettle.

The word ἐχίνος in this sense is familiar in connection with the Athenian court procedure, where it denotes, according to a scholion on Aristophanes (*Wasps* 1427) ἄγγος τι χαλκοῦν ἢ καὶ ἐκ κεράμων, εἰς δὲ καθίσαι οἱ διαίτηται τὰ γραμματεῖα τῶν μαρτυρῶν κτλ. Mark his "deposit"! Apart from the courtroom, the utensil was common enough to have its name borrowed by the Romans; for the *vilis echinus* not only appears among Horace's modest requirements in the way of household ware (*Sat.* 1. 6. 117), but was known to Lucilius long before him (*Marx* 1158). Porphyrio, who is responsible for

the citation from Lucilius, remarks that he used the word as if it meant a leather bottle (*ampulla*); "here however" (i.e., in Horace) "we must understand a glass bottle." But it certainly does not mean *ampulla* in extant Greek. In Callimachus (frg. 250 Schneider: *ἄγονται δ' οὐχ ἵππον ἀέθλιον, οὐ μὲν ἐχῖνον | βουδόκον*) it is a tripod-kettle, a λέβης, and a big one—"ox-capacious"! Pollux, Lucian's contemporary, defines it as a species of pot (*χύτρας εἶδος*, 6. 91), and farther on (10. 95) mentions ἐχῖνοι and ἐχινίσκοι among the "pots, dishes and pans and such other commodities" of the cook. The definition of Erotian in his glossary on Hippocrates is fuller, and accompanied by three references to comedy: *ἔστι δὲ ἐχῖνος χύτρας εἶδος μεγαλοστόμου καὶ μεγάλης. μέμνηται τῆς Λέξεως καὶ Εὐπολῆς, καὶ Μένανδρος ἐν Ἐπιτρέπονσι, καὶ Φιλῆμων ἐν Μυρμιδόσι*. Unfortunately, the reference to the *Epitrepontes* is not to the part extant in the Cairo codex. In the Hippocratean corpus, however, the word is still to be found; Eustachius, quoted in Franz's edition of Erotian, gives one instance from *de natura mulierum* (Kuhn II, 600) and three from *de morbis mulierum II* (Kuhn II, 843, 873, 875). We learn from them that it was used to decoct medicaments in, and when so used needed a cover. Furthermore, it is thrice specified (for *κενὸν* should be *καινὸν* in Kuhn 600; cf. 843, 875) that the vessel must be new. The reason is not given, and must therefore be the obvious one that an old pot would be too tainted for that purpose. This has no bearing on Lucian, but quite satisfactorily explains, I take it, the *echinus infectus* of Lucilius; it was not, as Marx thought, painted on the outside but tainted and no doubt incidentally stained on the inside by something cooked in it either as a condiment or as a drug. It was a pot, not a bottle; and in Horace, too, let us have done with bottles and dish-pans, and call a pot a pot, or anyhow a pipkin.

To return to Lucian, the Ionic sanction of "Hippocrates" makes the word especially apt for this piece; and as for the thing that it denoted, nothing could have been more suitable for the purpose of receiving contributions.<sup>1</sup>

It hardly needs emphasis that the error in transmission was natural and the correction easy from that point of view. The archetype of ΓΕ habitually confused *ει* and *ι*, and was written in uncials. A very similar, but, as far as the diphthong is concerned, opposite, mistake is in *Bis Accusatus* 14, where Γ has *φίλη* for *φείδη*. Such parallels, however, need not be heaped up, since the identical mistake can be documented, not in Lucian, to be sure, but in Aristophanes. In *Peace* 1086 for ἐχῖνον the scribe of the Venetus set down ἐκῖνον. Though a later hand has erased the middle syllable and corrected the word, the original writing is still legible even in the reproduction (fol. 143 a, line 6).

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<sup>1</sup> Compare Aleman 33 *Bergk*: καὶ ποκά τοι δώσω τρίποδος κῖτος ᾧ κ' ἐνι . . . ἀγέλης. May not τοι be a god, and ἀγέλης refer to offerings?

ON THE *AGERE-FACERE* ALDINE TEXT OF PLINY'S  
LETTERS

This note is prompted by one of Professor E. K. Rand (*C.P.*, XVIII, 348) on my preliminary notice (*C.P.*, XVIII, 68) of a double reading found in copies of the first edition (1508) of the text of Pliny's *Letters* by Aldus.

It is indeed well known that some early printers had only a small amount of type at their disposal, and printed their books sometimes perforce of this and other reasons only a page or two at a time, and that such impressions not infrequently show variations in readings. One instance, at least, has been cited where a single type had apparently been drawn from its place, presumably by the ink-dabber, and even left lying on top of the rest, so that the printed page shows the clearly impressed outline of its form. But Mr. McKerrow's imaginary picture (which Mr. Rand quotes) of the house-keeping and technical methods in an early printing-shop nevertheless strikes me as somewhat amusing.

The point in my mind, and I think in my words, merely was that such variations had not thus far (unless my knowledge and that of Mr. Voynich is at fault) been reckoned among the things to be expected in a *printing by Aldus*. The discovery of even this one suggests the advisability of caution in quoting readings as by Aldus, if they are taken, as probably everyone has been accustomed to take them, from any chance copy of a given Aldine "edition." I have even wondered whether Keil may not have been sometimes right, according to the book he used, in quoting the readings of Aldus that I have corrected in my critical edition of Pliny's *Letters*.

Mr. Rand says that I "seem inclined to assume that *agere* is the original reading" of Aldus' "copy" for the typesetter. I suppose he deduced this, in the fatal manner of seizing upon isolated proof-texts from Holy Writ, from a certain sentence toward the end of my note. He apparently disregarded altogether an earlier sentence in which I said that "it seems more likely that the book would be set up from some one of the recent printed editions, corrected and supplemented into shape; and all the preceding editions, so far as I have been able to test them, read *facere* in this place." This certainly might quite as well indicate, one would suppose, that I was "inclined to assume" the other of the two alternatives. For, now that we know that some copies of the printed text read *agere*, and others *facere*, no one (I should think) would be inclined to suppose that the former reading was deliberately and with full intention substituted for the latter, whether in the printer's "copy" or in the type. So long as we did not know that different copies of the book show this variation, we could but ascribe *agere* to a vagary of Aldus.

What I meant to do (I apparently did not do it well) was to avoid all assumption in the premises, but simply to indicate that difficulties attend the attempted explanation, whichever of the two readings be assumed to

have stood in Aldus' copy. I mentioned some (by no means all) of them. And I concluded my note by saying that "these are some of the puzzling questions that suggest themselves, and must remain unanswered, at least for the present." I meant, of course, unanswered by myself, until I had carried out my declared intention of collating the two volumes, one with the other, in which process other examples of a similar sort might be discovered.

But Mr. Rand promptly sat him down to answer the main question in the stead of my dilatory self, and I therefore anticipate here somewhat of that which I should otherwise have said later.

It is, of course, possible that an original *facere*, which stands in some (perhaps in most?) of the copies of the Aldine edition of 1508, was changed in certain copies to *agere* by some such process of accident and subsequent emendation of the remains as Mr. Rand suggests. But that process, or rather the assumption of it, raises too much difficulty to justify us in taking it as immediately and fully satisfactory. For if an *f*- had been accidentally withdrawn and lost out of the type of *facere*, presumably the vacancy would have been made manifest by the looseness of the remaining type, and such a "scholarly" typesetter as Mr. Rand postulates I should think would be prompted thereby to look for a missing letter. Such a superior person might even think of consulting his copy, which presumably would be available without great difficulty. It would appear very probable indeed that, if he observed the sign of loosened type, and was accustomed to such an accident in inking as Mr. Rand suggests, he would turn at once to the sheets already struck off, to see how the now mutilated word stood in them. But if he were content to trust only to his own editorial talent, I should think that the loose type would easily suggest the emendation of *-acere* to *facere*, that is, emendation by the insertion of an apparently missing letter, rather than by change of another. Even if there were no loose type to give him a hint of the correct process, and if he could not consult either the copy or the already printed sheets, I should still think it at least an even chance that he would correct *-acere* to *facere* rather than to *agere*. So much at present for the difficulty about Mr. Rand's suggestion.

Mr. Rand remarks that if my copy of the *agere*-text exhibits a certain appearance (I am not going to tell him here whether it does or doesn't), he might be led to espouse a certain hypothesis. But, for my part, I shall be driven to desperation indeed before I can believe and profess that the Aldine press may have set up and published in 1518 a new edition, or corrected issue, of its 1508 text, and yet have dated some copies of it, but only some, whether by inadvertence or by design, ten years back! It has, to be sure, been known that some entire editions of books have been put upon the market later than the printed date would imply. But this proffered parallel appears to me to be by no means a true parallel to such an extraordinary state of things as Mr. Rand, under compulsion, might assume for the given issue of the Aldine house.

Of course *agere* in the place in question must be classed among "emendations," if, as appears almost certain, it was a reading printed without manuscript authority. But I would not begrudge Mr. Rand the comfort he appears to find in thinking it is not to be viewed morally as "one of the perverse and wilful conjectures" of his illustrious client. Yet who knows whether Aldus did not fix the thing up with his own hands? And at any rate, *qui facit per alium facit per se*.

It might be mentioned here that I have turned over the task of collation, and of the consideration of the evidence that may be derived from it, to the willing hands and competent heads of two young doctorands who have had some training in my Pliny Seminar. Their work is already well advanced toward completion. Their conclusions, whatever they may be, will be their own, and will not necessarily agree with mine. They may even agree with Mr. Rand's. I have not said that my own would not.

ELMER TRUESDELL MERRILL

#### NOTES ON LATIN INSCRIPTIONS

##### a) CIL VI. 9685

Hung high on a wall of the Torlonia Museum at Rome (Tor. Album No. 97) is the famous relief in marble representing the interior of a market which Jahn (*Ber. Sach. Akad.*, 1861, p. 364) and others (see Reinach, *Répertoire de Reliefs*, III, p. 346) have called an advertisement. Above the head of the woman who keeps the shop there are inscribed the lines from *Aen.* i. 607-9, aligned as follows:

Dum montibus umbre [*sic*] lustrabunt

(c) onvexa polus dum sidera pascet semper honos nomenq.  
tuum laudesque manebunt.

These words, addressed by Aeneas to Dido with such fervor, seem somewhat inappropriate in this milieu of dead pigs and geese. Jahn's suggestion that they are ironical might appeal to one who goes marketing at present prices, but one is not accustomed to be met with such candor in shop advertisements. Indeed, the inscription seems to be not ancient. The side strokes of the letter *M* are entirely vertical, making a form which belongs to the third century or the renaissance, whereas the figures of the relief are carved in good second-century workmanship. Furthermore, the letter *B* in two cases has a large upper loop and in two instances a narrow one. Such irregularity is not usual in well-cut Roman inscriptions made at a time when practiced cutters followed models strictly. It is, of course, well known that renaissance owners of ancient reliefs and altars were in the habit of having inscriptions inserted and especially lines of ancient poets. This relief was first reported as being in the Giustiniana gardens, and epigraphers



are aware that several of the *falsae* now in the Vatican came from that collection (e.g., Nos. 3452-56 and 3625 of *CIL* VI. 5 [*falsae*], besides Nos. 3457, 3458, and 3627, now lost). We cannot draw any conclusion from the fact that the sketch in the Giustiniana catalogue does not show the inscription since Milesius' report of the inscription predates the catalogue, but it is possible that the maker of that catalogue had reasons for suspecting the genuineness of the inscription and therefore omitted it from his sketch. Be that as it may, I should, in view of the letter forms, the dubious provenance, and the inappropriateness, transfer the inscription from its respectable position in Volume VI to the collection of *falsae*.

b) *CIL* I<sup>2</sup>, 834

The Tomb of Bibulus, a part of which still stands near the Piazza Venezia in Rome, is assigned by Dessau (*Ins. Lat. Sel.* 862) to *septimo fere a.u.c. saeculo*, by Lommatzch in *CIL* I<sup>2</sup> (834) to about 100 B.C. and by Diehl, *Altlat. Inschr.* (493), to the middle of the second century B.C. This divergence of opinion is probably due to the fact that while the letter forms are very handsome and open and would satisfy all the criteria of Caesar's age, the archaic form *Poplicio* occurs, a form which is unusual even in the Gracchan period and carries the savor of the Ennian. When we observe that the stone of the handsome tomb is almost wholly of travertine, a material not liberally used till just before the Empire, we can only become very skeptical of the early dates usually given.

The only C. Publicius Bibulus known to history is, as Dessau remarks, the tribune of 209 B.C. mentioned by Livy (27, 20). But of course the inscription we have could not possibly be as old as that. What we have here is a restored inscription which gives the name alone in the original spelling of Punic-war days, modernizing all the rest. The normal orthography of *publice* at the end of the inscription indicates the divergence of dates between the original and the restoration. Judging from the material, the cutting, and the language, we ought to assign the restoration to about 60-50 B.C. In view of the tendency of recent scholars to regard inscriptions as wholly spurious when they reveal such inconsistencies, it seems worth while to call attention to one clear case of such a restoration.

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#### SOURCES OF PRUDENTIUS' *PSYCHOMACHIA*

Prudentius' *Psychomachia* stands at the headwaters of medieval and modern allegory—story-allegory as distinguished from allegorizing such as is in Philo-Judaeus' writings—and any inquiry into possible sources for this work takes significance from this fact.



In his poem, Prudentius turned into allegory the stories of Abraham's defending Lot against evil and of the renewal of Sarah's fertility; and the work is divided into two parts corresponding to these two stories: the contest of the Virtues against the Vices; and the speeches of Concord and Faith urging the need of peace and the building of a temple to Wisdom that Wisdom "may rule eternally."

Puech in his work<sup>1</sup> on Prudentius has quoted as a possible source an excerpt from Tertullian's *De Spectaculis*<sup>2</sup> in which various vices are pictured as conquered by various virtues:

Si scenicae doctrinae delectant, satis nobis litterarum est, satis versuum, satis sententiarum, satis canticorum, satis vocum; nec fabulae, sed veritates; nec strophae sed simplicitates. Vis et pugillatus et luctatus? Praesto sunt, non parva, sed multa. Adspice impudicitiam dejectam a castitate, perfidiam caesam a fide, saevitiam a misericordia contusam, petulantiam a modestia obumbratam, et tales sunt agones, in quibus ipsi coronamur.

Neilson<sup>3</sup> has quoted a similar one from Cyprian:

Ceterum quid aliud in mundo quam pugna aduersus diabolum cotidie geritur, quam aduersus iacula eius et tela conflictationibus adsiduis dimicatur? cum auaritia nobis, cum impudicitia, cum ira, cum ambitione congressio est, cum carnalibus uitiis, cum inlecebris saecularibus adsidua et molesta luctatio. obsessa mens hominis et undique diaboli infestatione uallata uix occurrit singulis, uix resistit: si auaritia prostrata est, exsurgit libido: si libido compressa est, succedit ambitio; si ambitio contempta est, ira exasperat, inflat superbia, uiolentia inuitat, inuidia concordiam rumpit, amicitiam zelus abscidit. cogeri maledicere quod diuina lex prohibet, compelleris iurare quod non licet.<sup>4</sup>

To these two passages should be added, I believe, another not noted hitherto, from Ambrose's *De Cain et Abel*:<sup>5</sup>

Duae enim mulieres unicuique nostrum cohabitant, inimicitia ac discordia dissidentes, velut quibusdam zelotypiae contentione nostrae replentes animae domum. Una earum nobis suauitati et amori est, blanda conciliatrix gratiae, quae vocatur voluptas. Hanc nobis opinamur sociam ac domesticam: illam alteram, immitem, asperam, feram credimus, cui nomen virtus est." . . . Voluptas tempts a man; Virtus comes to his aid. "Quem miserans virtus, et casurum cito videns; improviso occurrit, verita ne inter moras illecebris demulcentibus mens capiatur humana. 'Palam,' inquit, 'apparui tibi non quaerenti me. Ne fallat imprudentem, et circumueniat te mulier effrenata et luxuriosa

<sup>1</sup> Prudence, *Étude sur la Poésie Latine Chrétienne au IV<sup>e</sup> Siècle*, Aime Puech, Paris, 1888.

<sup>2</sup> *De Spectaculis*, chap. xxix. Migne, *Patrologia Latina*, Vol. I, col. 660.

<sup>3</sup> W. A. Neilson, "The Origins and Sources of the 'Court of Love,'" *Harvard Studies and Notes in Philology*, 1899.

<sup>4</sup> From *Cypriani Opera Omnia*, rec. G. Hartel, Vindobonae, 1868, p. 299. (*Corpus Scriptorum Ecclesiasticorum Latinorum*, Vol. III, Part I.) *De Mortalitate*, chap. iv.

<sup>5</sup> *De Cain et Abel*, Lib. I, Cap. IV and V. Migne P. L., Vol. XIV, cols. 322-26.

quae non novit pudorem.'" Virtus continues with advice about right living and suggests that if man really wants to drink and eat he may come to the feast of Wisdom: "'Sed vis manducare, vis bibere? Veni ad convivium sapientiae quae invitat omnes cum magna praedicatione.'"'

This passage is like the other two in that it contains an illustration of the virtue-versus-vice theme, here the contest of Virtus and Voluptas over man. On the other hand, its allegorical pattern is fuller, for the actors are distinct personifications. The main importance of the passage, however, lies in the fact that to the virtue-vice-theme is linked the praise of wisdom as the goal of a good life—an element which corresponds to the second part of the *Psychomachia*, but which is absent from both the other passages.

The quoting of this passage does not prove anything definitely, it is true, about what was behind the poem. But it shows a clear parallelism, and standing as a new passage beside those already noted, it helps us to see how fertile must have been the thought-soil from which Prudentius' work sprang.

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#### THE OCTAVIA ONCE MORE

The authenticity of our only complete Roman historical play has of late received not a little attention. In an article in 1920<sup>1</sup> I attempted to summarize the arguments favoring or opposing the theory that Seneca was its author and to defend its ascription to him. This I did, however, without knowledge of one important recent contribution<sup>2</sup> by Dr. Edwin Flinck, who had just vigorously espoused the same view. And to the same side of the question there was added, in 1922, the support of Professor Karl Münscher, in his study of the works of Seneca.<sup>3</sup> Later in the same year, however, in an elaborate review<sup>4</sup> of recent literature upon Seneca, Münscher published a palinode,<sup>5</sup> having been greatly influenced, as it appears, by the arguments of his colleagues Schöne<sup>6</sup> and Münzer.<sup>7</sup> Not a little of his second article is

<sup>1</sup> *Is the Octavia a Play of Seneca?* In *Class. Jour.*, XV (April, 1920), 388-403.

<sup>2</sup> E. Flinck, *De Octaviae Praetextae Auctore*. Diss. Helsingfors (December, 1919).

<sup>3</sup> K. Münscher, *Senecas Werke: Untersuchungen zur Abfassungszeit u. Echtheit* (in *Philologus*, Supplbd. 16, Heft 1 [1922], 126-42). On page 141 he concludes: "die erhaltene Oct. ist ein echtes Werk Sen.s, aber ein unfertiger Entwurf."

<sup>4</sup> In *Burs. Jahresb.*, CXCII (1922), 198-211. Hosius in his edition of the play (1922) gives no discussion of authorship, but Münscher (in his second article, p. 202) quotes a letter from him in which he inclines to Senecan authorship but is deterred by inability to accept the prophecy of Agrippina as written before the death of Nero.—F. L. Lucas (*Seneca and Elizabethan Tragedy* [1922], pp. 60-61) holds the *Octavia* to be spurious but gives no reason for his belief.

<sup>5</sup> Which on page 208 he declares to be preferable to obstinacy in error.

<sup>6</sup> See page 200.

<sup>7</sup> See page 203.

occupied with a review of stylistic questions, and, in spite of his recognition of Schöne's criticism that he had underestimated linguistic differences between the *Octavia* and the accepted plays,<sup>1</sup> he finally admits<sup>2</sup> that purely stylistic considerations offer no clear evidence against the genuineness of the play, since the language, thought, meter,<sup>3</sup> and composition of the *Octavia* are essentially Senecan.

It is by the historical allusions, then, that the question of authenticity must ultimately be decided, according to Münscher. And here the stumbling-block is usually found in the prophecy of the ghost of Agrippina (619-21, 629-31). As stated in my article,<sup>4</sup> which Münscher had not seen, the details foretold for the death of Nero are in part general, belonging to the conventional form of the curse,<sup>5</sup> and in part (for example, the *verbera*) quite inappropriate to Nero's actual experiences. Even the mention of his *ingulum* (629-30) as presented to his enemies, so often relied upon as an important evidence against genuineness, does not agree at all with the real facts as told by Suetonius<sup>6</sup> and Dio.<sup>7</sup> That anyone writing a prophecy after the event should not have made it conform more closely to the facts seems hardly credible.<sup>8</sup>

Again, lines 728 ff. refer, as understood by Miller, Flinck, and myself<sup>9</sup>—and by Münscher in his first article—to the stabbing of Crispinus, not that of Nero. Münscher's argument (p. 204) that one described as *trepidus* would be capable of slaying himself but not of despatching another, though he attempts to justify it by line 120, is unconvincing, for one may surely quiver with rage or tremble with excitement as well as from terror.

On pages 202-3 Münscher quotes the suggestion of Hosius that the *supplices* . . . *Parthi* of lines 627-28 may refer to the episode of Tiridates

<sup>1</sup> Pages 200 ff. The long discussion of the significance of the absence from the *Octavia* of compound adjectives in *-fer* and *-ficus* might have been spared had Münscher realized that, as stated in my article (p. 400), of the 31 adjectives in *-fer* in the Senecan tragic corpus not only the *Octavia* but also the *Troades* contains no example, and of the 12 in *-ficus* there is no example in the *Octavia*, the *Thyestes*, or the *Hercules Oetaeus*. Cf. also Flinck, *op. cit.*, p. 42, n. 2.

<sup>2</sup> Page 208: "es einzig und allein die historischen Beziehungen sind, die Sen. Verfasserschaft ausschliessen."

<sup>3</sup> On the question why the *Octavia* contains no lyric meter save anapaests cf. Flinck, *op. cit.*, p. 90; also Münscher's first article, pp. 135-36.

<sup>4</sup> Pages 389-91.

<sup>5</sup> *Ibid.*, p. 390, n. 3, in part following Siegmund, *Zur Kritik der Trag. Oct.* (1910, 1911). The account in Münscher's original article is much more logical and convincing than that in his retraction.

<sup>6</sup> Nero, 49.

<sup>7</sup> *Op. cit.*, 63, 29.

<sup>8</sup> Note also the strange omission, as observed by Gercke (*Fleck. Jahrb.*, Supplbd. 22 [1896], 196), of any reference to the events of 69 A.D.

<sup>9</sup> On pages 392-93 I have pointed out the great difficulties of interpretation which arise if it be supposed that Nero's throat is the one into which the sword is plunged.

in Italy in 66, a year after Seneca's death. But on pages 206-8 he admits (1) that the passage has been also referred to the events of 62, and (2) that even Seneca might have known of happenings such as to justify the use of the phrase. Yet that it refers to the Tiridates episode in 66 he deems more likely, on the basis of the statement (628) *regna divitias ferant*, citing (p. 207) Dio as evidence that Tiridates did not send his tribute to Rome but came in person with much wealth in his train. Surely this is to overstress the distinction of *ferant* (628) and *mittat* (626), two phrases used for variety rather than with any such subtle allusion in mind. Further, it is by no means certain that *regna divitias ferant* was in the mind of the author applied in any specific way to a particular sovereign or a definite occasion, any more than the preceding *mittat immensas opes/ exhaustus orbis* (626-27).<sup>1</sup> In the same context the allusion to Nero's Golden House (624-25), as Münscher admits,<sup>2</sup> might have been made by Seneca himself, and even the *armatae* . . . *cohortes* of lines 625-26 might, according to his concession,<sup>2</sup> have been written in the last days of Seneca's lifetime.<sup>3</sup> But the prediction may equally well be a vague and general one, and as such may date from a somewhat earlier period.

A more subjective argument is that on page 229, where Münscher finds the prophecy by Agrippina's ghost of the death of Nero a wish of Seneca's own heart but in contrast to the rest of the play, where the guilty Nero and Poppaea triumph, while the innocent Octavia passes to her death. But he fails, I believe, to analyze correctly the implication of the play as it stands. Had it been composed after the death of Nero, the tragedian, in his natural desire for poetic justice, could hardly have failed to use in a far more effective way the abundant material at hand to show how the death of Octavia was atoned for. The very fact that the allusions are few and vague is a clear indication that the play was written before any precise facts were or could have been known, when they were as yet but distant hopes, rendered a trifle more detailed by various poetic conventions on the one hand, and, on the other, by the prognostications of a philosophical mind speculating upon the future which might naturally await a tyrant like Nero.

<sup>1</sup> Cf. Pope's lines: "See thy bright altars thronged with prostrate kings,/While every land its joyous tribute brings [not 'sends']."

<sup>2</sup> Page 205.

<sup>3</sup> In this case, as I have recognized in my article (p. 398, n. 8), the likenesses between the *Octavia* and Lucan should perhaps be considered as borrowings *by* Seneca rather than *from* him, and the play might actually be the work upon which Dio 62, 25 says that Seneca was working immediately before his death: οὐ μέντοι πρότερον ταυτὸν ἤψατο πρὶν τὸ τε βιβλίον δὲ συντάξαι ἐπανορθῶσαι καὶ τάλλα, ἐδεῖλε γὰρ μὴ καὶ ἐς τὸν Νέρωνα ἐλθόντα φθαρῇ, παρακαταθήσθαι τισὶν. If the *Octavia* were the work here noted, Münscher's earlier view of it (cf. n. 3, *supra*) as an "unfertiger Entwurf" might approximate closely to the fact.

If all the stylistic evidence appears to such students as Hosius and Münscher himself to favor, or at least not to exclude, Senecan authorship, and if the historical allusions all admit of explanation on the theory that the play is a genuine one, doubtless posthumously published, then the defenders of its genuineness need feel no disquietude at the defection of Professor Münscher. In fact, I look in vain in his second article for any essential argument not already common property even before the writing of his first study, and a careful reading of his palinode leaves the impression that his conversion was a bit sudden and not at all points complete, as though his own judgment had been constrained by the *auctoritas* of another.<sup>1</sup>

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### A MEANING OF *RELIGIOSUS*

The discussion of the meaning of *religio* in W. Warde Fowler's *Roman Essays and Interpretations* has recalled to mind a special, legal meaning of the corresponding adjective *religiosus*, and something of its history. Several Latin dictionaries in common use either do not mention at all or inadequately state the meaning of the word. Gaius, who wrote in the period of the Antonines, in his *Institutes* ii. 3, gives the clearest definition:

Divini iuris sunt veluti res sacrae et religiosae. Sacrae sunt, quae diis superis consecratae sunt; religiosae, quae diis Manibus relictæ sunt. . . . Religiosum vero nostra voluntate facimus mortuum inferentes in locum nostrum. . . . Sanctæ quoque res, veluti muri et portæ, quodam modo divini iuris sunt. Quod autem divini iuris est, id nullius in bonis est.

This peculiar legal distinction between *sacer*, *religiosus*, and *sanctus* was in effect probably by the last century B.C., for Festus quotes from Aelius Gallus a statement similar to that of Gaius (Bruns, *Fontes Iuris Romani Antiqui*, 7th edition [Scriptores], p. 31).

A very nice illustration of this meaning of *religiosus* may be read in an inscription set up in a private cemetery by Phaon, a wealthy freedman, in

<sup>1</sup> After this note was in type there reached me three other denials of the Senecan authorship of the *Octavia*. That by L. Castiglioni, in *Rivista di Filologia*, LI (1923), 242, is a mere passing assertion, without argument; one by W. A. Baehrens, in *Philol. Wochenschr.*, XLIII (July 24, 1923), 668-71, repeats the old arguments with nothing new or not already answered; and the only novelty in that of Th. Birt (*Philol. Wochenschr.*, XLIII [August 4, 1923], 740-43) is the suggestion that, if we admit the play to be later than the death of Seneca, we may, by a change in the punctuation of line 628, interpret *regna* as an accusative, rather than a nominative. This arrangement, involving an asyndeton within an asyndeton and an unusually harsh use of the plural for the singular, few, I think, even among those who would detect in the line an allusion to the Tiridates episode in 66 A.D., will wish to adopt.

67 A.D.: "*Hic locus . . . sanctus religiosus est . . .* and it can neither be sold nor given away nor alienated." The whole inscription is worth looking up (*L'Année Épigraphique* [1914], No. 219).

With this definite meaning of the word applied to a place and tomb, it is rather odd that Cicero uses it often in an apparently contradictory sense in mentioning statues and temples of the *Dii Superi*. For example, in *Verres* ii, iv. 127, he speaks of a *religiosum simulacrum Iovis Imperatoris*; and in *Verres* ii, iv. 93, uses the expression, *ex Aesculapii religiosissimo fano*. An explanation of this use, such as it is, may be found in Festus: *Religiosum ac sacrum est, ut templa omnia atque aedes, quae etiam sacrae dicuntur; at quod per se religiosum est, non utique [sacrum est, ut sepulcra], quod ea non [sacra, sed religiosa sunt]* (Bruns, *loc. cit.*).

A citizen had the legal right to make a plot on his own property *religiosus*. This he did by burying a body in it, or by permitting a burial; but no person, who was not the owner, could make a spot *religiosus* by burial, except with the owner's consent. Curiously enough, the law even considered the case of the burial of parts of a body in different places, and decided that only the place where the head was buried became *religiosus*, the reason for this being that it is the *caput, cuius imago sit, unde cognoscimur!* (*Digest* xi. 7. 44). But even a cenotaph might be *religiosus* according to the jurist Marcianus, who cites Virgil as an authority for the opinion: *Cenotaphium quoque magis placet locum esse religiosum, sicut testis in ea re est Virgilius!* (*Digest* i. 8. 6).

The spot, by a burial made sacred privately to the *Dis Manibus*, was henceforth removed from trade; it could not be bought or sold. In the time of the Empire if any person desecrated such a place by removing bodies or bones he would be put to death if he was *humilioris fortunae*, while the *honestiores in insulam deportantur*. However, sanctity applied only to places under Roman sway. The law is stated by Paulus, a leading jurist of the third century: *Sepulcra hostium religiosa nobis non sunt; ideoque lapides inde sublato in quemlibet usum convertere possumus* (*Digest* xlvii. 12. 4).

The final stage in the history of the word and the law related to it is reached in Justinian's *Digest* in the sixth century. At that time the Empire was of course formally Christian. It is then interesting to notice that the compilers of the *Digest* in excerpting the passage of Gaius, quoted above, very carefully excised all mention of the *Dis Manibus* and *Dis Superis*. But this done, there remains no real distinction between *sacer* and *religiosus* so far as the meanings of the words alone go. The old terms nevertheless were kept, and apparently in the laws regarding a *locus religiosus* no change at all was made.

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## THE COMMITTEE ON MEDIAEVAL LATIN STUDIES

A committee on Mediaeval Latin studies with representatives from history, modern languages, philosophy, and classics, now organized under the American Council of Learned Societies, has just issued a bulletin reviewing its activities of the past two years and outlining its plans for the future. Any member of the American Philological Association who is interested and who has not received a copy may secure one by writing the secretary, George R. Coffman, 76 Oxford Street, Cambridge, Massachusetts.



## BOOK REVIEWS

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*Coins of the Roman Empire in the British Museum.* By HAROLD MATTINGLY. Vol. I, *Augustus to Vitellius*. London: British Museum, 1923. ccxxxi+464 pages. 64 plates. £3 3s.

The appearance of this volume must be greeted as an event of exceptional importance for the study not only of numismatics but of history in general. It is uniform with the familiar series of catalogues of the Greek coins and the coins of the Roman Republic, the Byzantine Empire, and the Vandals, Ostrogoths, etc., in the British Museum; and like them its value consists not only in making accessible to students one of the richest collections in the world but in contributing substantially to the interpretation of the numismatic material in question. It is well known that a new epoch in the study of Greek coins began half a century ago with the early volumes of the Greek series, and it is evident that the Roman imperial catalogue is destined to serve as a similar milestone on the road of progress.

The scientific study of the coinage of the Roman Empire has suffered from the very vastness of the material and the diversity and complexity of the civilization and the movements to which this material bears witness. No other numismatic series has been so familiar to scholars and to amateurs since the days of the Italian Renaissance; no other ancient coins appear to the beginner so easy to deal with; but nowhere else has the scholar in recent years felt so ill at ease in making use of his material. For it has been no secret that the copious volumes of the industrious Cohen, however convenient they may have proved to dealers and collectors, fell far short of the standard required in a scientific publication. This unhappy situation, however, was not to endure: and the volume under review puts us, with regard to the momentous period of a century in which the Principate was gradually assuming its definite form, on fully as satisfactory a footing as we are for the ages that precede.

The way for the present work was to some extent prepared by the arrangement of the British Museum trays which was carried out many years ago by the late Count de Salis and also by various articles in journals by Mattingly himself and his British colleagues, as well as continental scholars, especially Laffranchi; but it is probable that few outside a limited circle of specialists have had any conception of the clearness with which many details of the

imperial administration would emerge from the rigid scrutiny and comparison to which the coins have been subjected.

The carefully arranged plates render adequate stylistic appreciation possible not only for individual pieces but for series. The attribution of whole groups of emissions to the mints of the provinces is of fundamental importance. In many instances the description or interpretation of reverse types has yielded results of general interest, as in the case of the Augustan Temple of Concord. The indices are invaluable. The 231 pages of Introduction will serve to render accessible the information contained in the body of the catalogue.

The following extracts may serve as typical of what is contained in the Introduction: "The question of the origin of the Imperial system of coinage is really only a fraction of the larger question of the development of the Roman Empire out of the Roman Republic." "The civil war between Caesar and Pompey hastened the development that was already taking place." "When Actium gave Octavian the supreme control of the Roman world, the coinage had reached a condition in which reform was urgently necessary." "The imperial coinage of gold and silver is the direct successor, not of the Senatorial mint of Rome, but of the coinage of the 'imperator,' and after him, the triumvirs in the provinces. Octavian, as emperor, issued no purely imperial coins in Rome at all; he did not, in fact, inaugurate the imperial mint of Rome." "When Nero . . . began to administer the state in person, the coinage shared in the general change." "The revolt of Vindex and Galba against Nero in the Spring of A.D. 68 led to some interesting developments in coinage."

There are sections on Countermarks, on the Monetary System of the Empire, Fabric, Epigraphy, Art, the Types, Finds, and the distressing topic of Forgeries; a select Bibliography; and then a series of special introductions to reigns, well adapted to extract the full amount of historical information contained in the coins—in effect, a numismatic history of the period.

In the presence of a volume such as this, the ordinary canons of the reviewer's task cannot be applied: for who save the author himself has earned the right to discuss the technical side of this mass of precise observation and close deduction? Grateful recognition, however, it is proper to express; for the book renders invaluable service in placing the coins and their unique evidence within reach of the student. Moreover, the numismatic material has so many points of contact with broad historical interests that a perusal suggests not a few lines of reflection, some of which appear to deserve expression here.

The clearing up of the matter of the several mints is most helpful. With this aid, it is now possible to discern the individual tendencies of Spanish,

Gallic, Italian, and Eastern art. A profitable theme for further investigation would be the corresponding characteristics of local schools in the remains of their sculpture and minor arts; for Gaul, especially for Gallic bronze work, considerable is already known. This same distinction of mints has its bearing on portraiture, since the less reliable representations of the provincial mints are now to be kept apart from those more dependable portraits executed in the capital.

Again, the countermarks, with the hints they afford as to turns of political fortune and shifting of economic equilibrium, have an interest all their own; the purpose of such devices in general is "to review the coinage and mark such coins as pass the test," and so they are frequent, e.g., in the troubled times just before and after the death of Nero.

Future students of the *Histories* of Tacitus will be well advised to accompany their reading of the author's text with an investigation of the coins of the period: here they will find significant and suggestive types and legends such as the *Concordia Provinciarum*, *Libertas P. R.*, *Roma Renascens*, *Salus Gen. Humani* of Galba; the *Pax Orbis Terrarum* and *Securitas P. R.* of Otho; and the *Fides Exercituum* and *Libertas Restituta* of Vitellius; they will observe that under Otho there was no coinage of the base metals—this lay in the sphere of the senate, and "the Senate certainly had no love for Otho, and must have found some more or less plausible excuse for deferring the issues of *aes*"; and they will find a special attraction in tracing the extent of the control exercised by rival claimants for imperial prerogative in different parts of the west, as reflected in the issues of the mints of Tarraco, Lugdunum, and Rome. And then the portraits! Masterly are the delineations they will find of Nero wearing the radiate crown of the Sun-God; the worn, aged features of Galba; the neat, dandified Otho; and the coarse, bloated Vitellius, the antithesis in body as in mind to that remarkable prince with whom this extraordinary numismatic portrait gallery has its beginning.

A. W. VAN BUREN

ROME

October, 1923

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*The Roman Republic, and the Foundation of the Empire.* By T. RICE HOLMES. Three volumes. Oxford, 1923. \$21.00.

Despite its inclusive title this work of 1,440 pages is in reality a military history of the Ciceronian period. As such, if appropriately rechristened, it deserves to become a standard work. The author's enthusiasm for military topography, campaigns, strategy, maneuvers, and marching records is contagious, and since military history fell into undue neglect before 1914, there is a legitimate place for a work like this, now that gruesome experience has compelled us to admit that wars still have a place in history. Nowhere

else will the student find such a lucid account and convincing interpretation of the campaigns of Lucullus, of Pompey and of Caesar in Gaul, Greece, Egypt, Asia, Africa, and Spain. The descriptions of the battles of Alesia and of Pharsalia are worthy paraphrases of the classic original. Readers of his previous books will expect accuracy, full documentation and numerous thoroughly argued appendices, and they will not be disappointed. Dr. Holmes has carefully sifted practically all pertinent essays and dissertations in whatever languages written, and has usually reached sound conclusions, though of course in issuing a work so extensive he could hardly consider books of the last three or four years except in summary (sometimes too impatient) treatment relegated to footnotes.

However, when Dr. Holmes leaves his favorite theme he is disappointing. The second chapter, entitled "The Roman World in Cicero's Day," is so feeble that it will probably repel many readers before they reach the more valuable portions. He leaves the impression that he knows Rome only as pictured by the historians, that he has not read discerningly in all the rest of Roman literature, that he has not penetrated deeply into Roman institutions and civilization, but has contented himself with culling a few general facts from uninspired handbooks. The Catilinarian conspiracy is well enough handled because the author knows how to extract the gist of Sallust and of Cicero's speeches, but the political history of the critical years 60-50 B.C. loses itself in trivialities. It gives no convincing portraits of the chief actors, Caesar, Pompey, and Cicero, and it makes no adequate attempt to diagnose the symptoms of dissolution. However, one is grateful at least that Dr. Holmes avoids Eduard Meyer's error of fitting events into a strained hypothesis. He jots down the facts for what they are worth and eschews theorizing.

The weakest chapter is perhaps the last. Here, where we expect an estimate of Caesar's character and work (for after all Caesar is the hero of these volumes) and a just analysis of his failure to carry the senate into the imperial program, the narrative languidly seeps away into the sand, and that is all. Caesar's enduring contribution in ideas which he had not the time to carry into execution, the deep meaning of the imperial cult which he projected, the fundamental social changes which made the revolution possible are subjects barely hinted at. These limitations seem to be due to a personal bias which in one instance leads the author to make an impatient attack upon *Quellenforschung*. "What will he have gained," he asks (I, 338), "when he has decided whether Plutarch and Appian used the lost work of Asinius Pollio directly or only through a Greek medium?" For one thing he will have learned that Appian is reliable in treating events of 50-44 B.C. and not thereafter. In his military history Dr. Holmes is excellent because, a student of tactics, he can check muddled sources by the logic of military rules; but in the social and political history where one cannot apply such

criteria one must know the relative worth of one's sources. Despite such limitations the work will prove indispensable for the portions in which Dr. Holmes proves himself a master.<sup>1</sup>

TENNEY FRANK

*Philologische Untersuchungen.* Herausg. von KIESSLING-WILAMOWITZ. Achtundzwanzigstes Heft: "Plautinisches im Plautus." VON EDUARD FRAENKEL. Berlin: Weidmann, 1922. Pp. 435.

This work is the most valuable contribution made to the study of Roman comedy since the publication of Leo's *Forschungen*. A pupil of Leo's, Fraenkel is almost the equal of his master in the control of his material and out-Leos Leo in the ingenuity of his methods. The results, however, are often at odds with Leo's own conclusions. In fact, the book marks an important swing of the pendulum away from emphasis upon the Greek models toward a precise and liberal estimate of Plautus' originality. The method may be described as cumulative: starting from definite ideas and forms of expression that are constantly recurrent in the plays the author discovers the favorite turns of speech and thought of the Latin poet and then proceeds to build upon these apparently trivial details a superstructure of important conclusions regarding Plautine expansions of monologue and dialogue, his refashioning of scenes and whole plays, and his inventive power in the cantica. A final chapter summarizes the distinguishing features of the poet's originality.

Naturally, critical attention must be directed toward the substructure that supports these large conclusions. The text of the discourse is *Casina* 759 ff. At the beginning of a monologue a character declares that the *ludi* (tricks) played on the old man of the play are jollier than the *ludi* (games) at Nemea or Olympia. The word play in *ludi* is wholly Latin and the thought Plautine. The Nemean and Olympian games were as commonplace to the Roman of Plautus' day as to the Greek. The whole passage, therefore, is Plautine and not necessarily prompted by any reference in the Greek original to Greek games. Now this passage represents the *ludi* of the intrigues as surpassing, not merely equaling, the Greek *ludi*, and Fraenkel accumulates a number of similar comparisons in Plautus, many of them comparisons with Greek persons and things and largely with the facts and persons of Greek mythology, in which as in the *Casina* the object or person compared regularly surpasses the Greek illustration. So Toxilus at the beginning of the *Persa*

<sup>1</sup> The recantation of my dating of the invasion of Italy, which Dr. Holmes requests (II, 337), I have already given in my *History of Rome*, p. 295. Dr. Holmes's arguments are valid in part, but here, as elsewhere, he discharges some fuseless shrapnel. Why could not Curio ride 90 miles a day for three days when, as he says (*ibid.*, p. 18), Caesar did so for a week? Was Caelius a tribune in 50? How does he know that Cic. *Fam.* viii. 17. 1, refers to January 7, or that *Arimini* must be changed to *Ariminum*?

announces that the lover *superavit aerumnis suis aerumnas Herculei*, and there follows a list of Hercules' labors, one of which is erroneously ascribed to the Greek hero. Leo had previously stated, on the basis of the error in mythology, that Plautus added the list of labors, but Fraenkel, having found *superavit* to be a touchstone by which to recognize a Plautine idea and phraseology, leaves nothing to the Greek original but the opening verse of the play and claims for Plautus the second verse, quoted above. The first chapter assembles such *komparativische Gesprächsanfänge* and through the elements of style and expression common to all of them establishes even the Greek allusions in them as Plautine fancies rather than inspirations from the Greek originals. This theme leads to the related ideas of metamorphosis and identification, as expressed in *Herculem fecit ex patre* or *muscast pater*, to mythological allusions of an extended range, such as the monody of Chrysalus in the *Bacchides*, and finally to the "animating of the inanimate" in *vox advolavit* or *vocem volucrum*. In many such ideas and expressions Greek words and allusions appear, but the idea and the phrasing stamp both the phrasing and the Greek ingredient as Plautine in origin. So a vast amount of Greek matter which hitherto has served as underpinning for theories of dependence on Greek models falls away, and by a careful examination of recurrent ideas and phrases Fraenkel builds a new foundation on which he may firmly establish his belief in the rich inventiveness of the Latin poet. At every turn the particular idea or word is not found in Terence or in the Greek fragments, but is widely employed by Plautus.

From these four chapters there results a collection of invaluable material for the student of Latin style, idiom, and phrasing. And the presentation of the subject is so attractive and so ingenious that the reader is almost swept off his feet and persuaded of the truth of the conclusions. A few objections may help such a reader to suspend judgment at least. The fact that Plautus with such uniformity in his comparisons is not content to say that *A* equals *B* but insists that *A* surpasses *B*, fits very well the generally accepted notion of exuberance and extravagance as conspicuous features of the Roman poet's personality. But that mythological comparisons of this type are wanting in the fragments of Menander (p. 12) does not go far toward establishing that the scheme of expression is Plautine. For Greek literature, apart from Menander, is reasonably full of such hyperbole. Herondas ii. 90, for example, *ταῦτα τρυτάνη Μίνως οὐκ ἂν δικάζων βέλτιον διήγησε*, is on the same plan, and Headlam's note *ad. loc.* quotes twenty or thirty examples from all over Greek literature, a list easy to increase. Fraenkel, in passing to non-mythological comparisons dwells on *Casina*, 225, *munditiis munditiam antideo* (p. 14), but if Meleager (*A.P.* v. 148) has *νικάσειν αὐτὰς τὰς Χάριτος χάρισιν*, the absence of such formulas from Kock's two volumes of Greek comic fragments will fail to convince me that there is much solid evidence of originality in Plautus' use of it. Such depreciatory comparisons are commonplaces, whether mythological or not, and one should not use cases of *supero*, *antideo*,



etc., as touchstones of Plautine phrasing or of Greek phrasing although the large amount of Greek mythological allusion in connection with them might well dispose any student to just the opposite of Fraenkel's conclusions. So far as they often appear at the beginnings of monologues, that feature is as likely to indicate conventional stereotyping in the Greek originals as stereotyped diction and thought of Plautus himself.

The argument from the silence of the Greek fragments is always dangerous. There was a large number of Greek poets, and many of them were very productive playwrights. Even in so apparently clear a case as *tondere* (p. 74, cf. p. 213), which in its common sense of "cheat, swindle" seems to be Latin exclusively while Greek *κείπειν* stops at the figurative sense of "plunder, pillage" with "cattle" and "land" as its objects, the schoolboy of Herondas' third mime (vss. 38-39) who *τὴν μάμμην κείπει*, "sponges on his granny," brings the word perilously near the force of *tondere* in Plautus. Yet on this word alone Fraenkel bases large conclusions.

The later chapters indicate the nature of Plautus' expansion of monologue and dialogue, using the words and ideas studied in previous chapters as mintmarks of Plautine composition. In the course of this cumulative study other features emerge as conspicuously Plautine, such as scurrility, military language whether literal or figurative, and by these new touchstones Fraenkel thinks he discovers Plautine workmanship in expansions of the rôles of the intriguing slave and parasite. From studies of such scenes he passes to the plays as wholes, and in his chapter on contamination, professing to be neither an adherent nor an enemy of the modern school, he reaches negative conclusions: Plautus did not take organic dialogue-passages from different plays and weave them into a new whole, or invent independently elements of the main action, but his procedure in general was that of Terence in the *Adelphoe*. In a chapter on the cantica he demolishes Leo's theory that Plautus was inspired to write his song measures through personal acquaintance with Hellenistic songs in the theaters of Southern Italy, and argues that Roman tragedy had already introduced such songs, although Plautus' creative genius increased and varied the forms of artistic dramatic lyric so that, properly enough, the *numeri innumeri* mourned his death.

The ultimate conclusion may be found in the characteristic German sentence which describes Plautus' dominant quality as "die auf die Intensität des sinnlichen Wahrnehmens ruhende Fähigkeit das Charakteristische eines äusseren Vorgangs, einer Bewegung, eines Geräusches oder dergleichen bis in die leisesten Nuancen hinein aufzunehmen, das Aufgenommene in der Phantasie jederzeit zu reproduzieren und dann dafür reiche und erstaunlich präzise sprachliche Ausdrucksmittel von suggestiver Kraft zu finden."

Whatever reservations regarding the details I have expressed, I value highly the rich contribution made to our appreciation of the Latin language. Why should we study the relation of Plautus to his Greek originals, con-



tamination, and the like when these plays offer such rich opportunities for appreciating the niceties of Latin expression and style? Fraenkel's collection of passages covering recurrent ideas and phrases is much more important and helpful than his attempt to determine from them the original Plautine element in the plays.

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*Die Pädagogik des Isokrates.* Von AUGUST BURK. Studien zur Geschichte und Kultur des Altertums, XII. Band, 3-4 Heft. Würzburg, 1923.

In an age of fancy-free pedagogy like our own, when many advanced thinkers not only believe the student is a king who can do no wrong, but also incline to despise platitudes, particularly ethical, it is interesting and instructive to remember Isocrates again. Critics from Dionysius and Hermogenes to Macaulay have usually begun by assuming that Isocrates, the father of pedagogical platitudes, should be Plato or Demosthenes, and have ended by despising him because he is neither. This mistake the author of the essay under consideration does not make. He is, indeed, inclined to over- rather than underestimate Isocrates.

Beginning with an introductory section, Burk weighs the work of his predecessors in this field, Büchle and Strowski (*Die Pädagogik des Is.*, Progr., Baden-Baden, 1873, and *De Isocratis paedagogia*, Albi, 1898), and finds them wanting in breadth of vision and in that sympathy with the sophistic movement which are needed for the right understanding of Isocrates. He next sketches the relations of Isocrates with the sophists and the Socratic circle, estimates the contributions made to the system of Isocrates by the ethico-political speculations of his forerunners, briefly discusses educational conditions in Greece of that day, and summarily reviews those writings of Isocrates which he proposes to use as sources. These are, in short, all the extant works of Isocrates save the forensic orations. On this basis Burk builds up in the second part of his dissertation, and not without much speculative timber, the pedagogical system of Isocrates. Save where he compares the views of Isocrates with the theories of modern writers on education, or where he manufactures evidence out of emotion, Burk has little that may not be found in the pages of Jebb and Blass.

Burk's main thesis is that we may take as typical of the instruction carried on in Isocrates' schoolroom the aphorisms or general maxims that we find in the orator's published writings. Such an assumption when we are dealing with the works of an author who by his own confession (*Panathenæus* 200) revised his compositions with his students, is perfectly natural, but at the same time dangerous. It leads to speculation, and Burk has not always managed to separate speculation from evidence. However, it is ungracious

to find fault in this manner with an essay which is so uniformly interesting and vividly conceived. That, when one is dealing with Isocrates, is no mean achievement.

The third part of the work is, to the mind of the present writer at least, inadequate. It concerns the influence of Isocrates on the pedagogical theory of later antiquity, of the Renaissance, and in part of modern times. In view of the excellent and full account of Isocrates' theories presented in Blass and Jebb, and inasmuch as the history of Isocrates' influence in the Renaissance has yet to be written, it would seem that more than twenty-odd pages out of two hundred and twenty-four could have been spared for this task. The book is made more useful by the addition of two indexes (Namenregister, Sachregister.) No doubt it will be of great interest to students of the history of oratory and of education, and will take its place among the standard essays on Isocrates.

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*Euripides and His Influence.* By F. L. LUCAS. "Our Debt to Greece and Rome" Series. Boston: Marshall Jones Company, 1923.

Mr. Lucas' volume is one of the best in the series, and one of the few that fulfils the promise of its title. There is a brief characterization of Euripides in thirty-eight pages and the remainder of the book is a study of his influence. It is, of course, from a point of view of heavy erudition a slight performance. But that is not the fault of the author, or of anybody. He has done as much as was possible in the limitations imposed. He tells more of literary history than most other volumes in the series and much more than the corresponding volume of Steiger in the German series "Das Erbe der Alten." And he tells it enthusiastically, even eloquently. I do not share his enthusiasm or that of other Euripides fanatics, and shall some time try to prick the bubble. The sober truth, of course, is already in print in Jebb's article in the *Encyclopaedia Britannica* and the chapter in his *Lectures on Greek Poetry*. Like other Euripideans Mr. Lucas pays heavily for his one-sided devotion to his idol in neglect, coldness, contempt, or faint praise for Aeschylus, Sophocles, Plato, Pindar, and Aristophanes. The price is too high.

PAUL SHOREY

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*Virgil and His Meaning to the World of Today.* By J. W. MACKAIL. Boston: Marshall Jones Company, 1922.

Starting with a proud but gracious assertion of Virgil's place as a world-master, Mr. Mackail proceeds to give with admirable economy the necessary

political and literary setting to his life and poems. His next step is a concise life of Virgil. After this come chapters on the Eclogues and Georgics, including a brief non-technical discussion of the disputed youthful poems. Throughout these early sections Mr. Mackail prepares the way for the story of Virgil's concentration on the epic, the growth and structure of the *Aeneid*, and the Italo-Roman ideal. These are the high spots in the book but the concluding chapters do not slump. On Virgil in the medieval and modern world, and on style, diction, and the Virgilian hexameter, Mr. Mackail contrives to be interesting and non-technical enough for the unlearned reader.

This volume sets a superlative standard for the series. It has structure; the style marches and often sings. The picture of Virgil and his times is artfully retouched with modern equivalents, but only where they are absolutely equivalent. The elements of widest popular appeal, such as the workshop methods of a master poet, are emphasized without slighting the demands of aesthetic criticism. Where it was essential to his point Mr. Mackail has not hesitated to quote Virgil's lines without translation. His judgment, we believe, was sound. The citations appeal to the lettered reader; not numerous enough to embarrass that timid creature, the general reader, they might even serve to bait him into getting himself a little Latin.

This book should have a wide usefulness. Not only does it serve as a general introduction to Virgil but the sections on the *Aeneid* are sufficiently ample to make it an admirable companion to the study of that poem in schools and colleges. Last but not least, it is a model of sound but popular literary criticism.

KEITH PRESTON

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*Aeschylus with an English Translation.* By HERBERT WEIR SMYTH.

"Loeb Classical Series." London: William Heinemann; New York: G. P. Putnam's Sons, 1922. In two volumes. Volume I: "Suppliant Maidens," "Persians," "Prometheus," and "Seven Against Thebes."

Professor Smyth's lifelong study of Aeschylus designated him as the Loeb translator. He has not disappointed expectation. Without overburdening the footnotes he has used his expert knowledge to give us a sane, conservative text with sufficient indication of all departures from the readings of the Medicean, discrepancies between the Medicean and other manuscripts when important and the sources of all emendations accepted. It is a more reasonable text than that of Wilamowitz. The Introduction is slight, but sufficient. It aims only to give the necessary information. Appreciation and literary criticism are apparently reserved for another occasion. The translation, it need hardly be said, is correct—though of course there will always remain passages about which difference of opinion is permissible. The

diction is a happy compromise between commonplace and consciously poetical prose. It is just sufficiently heightened to give the English reader some notion of "Aeschylus' bronze-throated, eagle bark at blood" without requiring him to strain at unnatural English. Professor Smythe has laid many previous translators and interpreters under contribution. But the result is his own. He rarely if ever offends, which is no slight praise for a translator of Aeschylus.

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PAUL SHOREY

*Die Begriffsform im Mythischen Denken.* By ERNST CASSIRER.  
Leipzig and Berlin: B. G. Teubner, 1922. \$0.24.

This thoughtful, but abstract, essay belongs rather to philosophy than to classical philology. Beginning with a sketch of the development of logic in relation to mathematics, the writer traces the origin of the conception of a logic of the *Geisteswissenschaften* to Vico and answers affirmatively the question whether there may not be also a logic of unscientific and mythological thinking. The very etymology of logic with its reference to language seems to imply that all reflections on the processes of thought and the association of ideas through words is a kind of logic. The author develops this suggestion with illustrations from totemism, astrology, and the philosophy of nature from the Renaissance down. He quotes freely from the literature of sociology and anthropology, including English and American sources. The essay is the enlargement of a Hamburg lecture and a sketch preparatory to a "Philosophie der symbolischen Formen" which the author hopes to publish soon.

PAUL SHOREY

